The Logic of Discovery

By

R. D. CARMICHAEL

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DEDICATED TO MY PARENTS

DANIEL MONROE CARMICHAEL

AND

AMANDA LESSLEY CARMICHAEL

WHOSE CHARACTER AND SOUND JUDGMENT DIRECTED
ME TO A WAY OF LIFE WHICH MAKES LIFE
WORTH LIVING

PREFACE

The modern critical scrutiny of the foundations of mathematics has raised far-reaching questions concerning the nature of the process of discovery in all exact scientific investigations—questions which I have sought to discuss in some main aspects in this volume. No technical knowledge of mathematics on the part of the reader is presupposed. His intelligent interest in the progress of thought and his willingness to proceed consecutively from conclusion to conclusion is all that the following exposition will demand of him.

In the preparation of the book I have utilized certain previously published articles of mine. From *The Monist* I have taken: "The Logic of Discovery" (October, 1922); "Concerning the Postulational Treatment of Empirical Truth" (October, 1923); and "The Structure of Exact Thought" (January, 1924). From *Scientia* (December, 1923) I have taken "What is the Place of Postulate Systems in the Further Progress of Thought?", and from *The Scientific Monthly* (May, 1922) "The Larger Human Worth of Mathematics." To the editors of these journals I am grateful for the privilege of utilizing these articles here.

To several friends I owe special thanks for the stimulation and encouragement which I have received toward the preparation of this book through their interest in it. My greatest specific obligations are due to "The Philosophical Club" of the University of Illinois, whose weekly discussions of the philosophy of science have been of much value to me. The first chapter of the book was prepared for and read as a presidential address before this club and a group of invited guests.

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R. D. CARMICHAEL.

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CHAPTER I

THE LOGIC OF DISCOVERY

§1. *Ways of Discovery*. It is conceivable that in the nature of things the mind is unable to comprehend its highest movements with clearness. If the act of discovery or invention, in its rarer and more profound phases, is, as it seems to be, one of the highest experiences of the mind, it would then be natural to expect that the mind itself could not explain such an act of invention or discovery. If any portion of the mental processes should thus lie beyond the reach of scientific analysis, one would have a domain in which precision of thought could not be attained. This looks too much like a mystery to be accepted without repugnance. But there seems to be a real difficulty in supposing that the mind can explain, or comprehend clearly, all of its acts. If so, it could explain also the act of explanation, and then this act in turn, and so on, apparently with an infinite regression. Whether such impossible infinite regression can or cannot be avoided, it is clearly a conceivable possibility that certain mental processes can not be fully apprehended by the experiencing agent, so that one must not assume in advance that the mind can certainly comprehend clearly all of its own movements.

But this conceivable possibility need not at all affect the development of a logic of discovery in the sense of a logic by which one infers from the known to that unknown which hitherto has not been apprehended or suspected. It is with the laws of such inference that a logic of discovery will be concerned. The data from which one starts and the conclusions reached through the use of these laws will be clearly apprehended in their relation to the process; and the completed act with its several steps can be held up, as it were, before the mind for inspection and analysis. The processes of thought in such discovery may be studied objectively after the act by means of the remembered steps of the inference; and the laws of such inference may be successfully investigated.

But it must not be assumed in advance that all the processes of discovery are carried forward by means of a logic—the inference from known to unknown. It is conceivable that any logic of discovery will necessarily leave out of account some of the most characteristic acts of discovery. One might prefer to avoid this conclusion, but one must be prepared to face the possibility. As J. A. Thomson has said: "It may be that the imaginative brooding suggests a solution in some way that we do not at present understand—life is essentially creative; it may be that there is a more or less unconscious cerebral experimenting; it is certain that letting the mind play among facts has often led to magnificent conclusions. It seems that the solution is often reached first and the proof afterwards." There is the conceivable possibility of an actual creative activity of the intellect which is not suitably analyzed in terms of any logic of inference from the known to the unknown. It is clear that many of us wish to have our universe so tidy that nothing actually novel could happen in it. But there are others who would not object to the surprises of true novelty, who indeed would be pleased with them rather than disconcerted by them. But the matter is controlled by the wishes of neither group. The question is one of fact, hard to be ascertained perhaps, and not one of opinion.

In this also, whatever the truth may be, there is no effect on the development of a logic of discovery. For such creative acts, if they really take place, are outside of the category of inferences from the known to the unknown; and hence lie outside of the domain of a logic of discovery.

To our definition of the logic of discovery as the science of inference from the known to that unknown which hitherto has not been apprehended or suspected, we may add a few remarks as to what it is not by way of more clearly delimiting the meaning of the term. In the first place it is not necessarily a logic of demonstration. One may discover a truth by means of a definite process of inference which leads forward by well-defined steps to a clearly ascertainable proposition without carrying with it a demonstration of that proposition. This is often done by the mathematicians in important ranges of investigation. An actual demonstration of the results thus heuristically attained is then frequently given in a *de novo* argument. If some particular type of inference which is thus often successful should turn out to be always so, in the sense of never leading to false conclusions, one would suspect that a more careful analysis of it would reveal the fact that it could be put into the form of an actual demonstration. But some of these heuristic methods, which are successful in yielding true conclusions when they are sagaciously employed, sometimes lead one also to formulate propositions which turn out to be false. They give a method of discovery which must be checked by a subsequent demonstration of the results. Such a method affords an example of a logic of discovery which is not a logic of demonstration.

We have already considered the possible existence of a creative method of invention which cannot be treated by a logic of discovery, the latter being confined to inferences from the known to the unknown. Such a logic, then, does not take account of all things, even of all important things, relative to discovery. Some of these may belong to the psychology or the physiology of the investigator. The logic of discovery has to do only with certain laws of inference. If we should think of the body of known truth in a given field as being inclosed in a region beyond the boundary of which lies what is unknown, it might be an important question whether one is most likely to be able to break over this boundary if he approaches it on a gradual spiral path taking him through a large part of what is known already or along a radial path quickly bringing him squarely against the boundary; such a question, whether important or not, would have nothing to do with a logic of discovery. It belongs rather to the psychology of investigation relative to the particular domain of truth. The logic of discovery has to do only with the laws of inference from the known to the unknown.

When we notice to what extent discoveries in science appear to be made in unforeseen ways and how often an accidental juxtaposition of thoughts leads

the detection of something new, we feel that it would be hazardous to miss one link from the chain of scientific progress or leave out of our thought even the least inkling as to what may be a successful logic of discovery. We have not yet learned how to systematically explore new territories of thought. We can only look over the field at random and hope to find here and there a pearl of great price. In the absence of a guiding logic of discovery we have no systematic method of procedure.

Francis Bacon has emphasized this matter, saying, "So it cannot be found strange if sciences be no farther discovered, if the art itself of invention and discovery hath been passed over. That this part of knowledge is wanting, to my judgment standeth plainly confessed; for first, logic doth not pretend to invent sciences, or the axioms of sciences, but passeth it over with a '*cuique in sua arte credendum.*'" He says further: "So it should seem, that men are hitherto rather beholden to a wild goat for surgery, or to a nightingale for music, or to the ibis for some part of physic, or to the pot lid that flew open for artillery, or generally to chance, or anything else, than to logic, for the invention of arts and sciences."

Descartes also speaks much to the same tenor; he says: "I found that, as for logic, its syllogisms and the majority of its other precepts are of avail rather in the communication of what we already know or even... in speaking without judgment of things of which we are ignorant, than in the investigation of the unknown." As to places where he might find correct reasonings, Descartes writes: "For it occurred to me that I should find much more truth in the reasonings of each individual with reference to the affairs in which he is personally interested, and the issue of which must presently punish him if he has judged amiss, than in those conducted by a man of letters in his study, regarding speculative matters that are of no practical moment, and followed by no consequences to himself." But the primary source of that method which is so clearly described in Descartes' classic *Discourse* is in mathematics. He gives the following account: "The long chains of simple and easy reasonings by means of which geometers are accustomed to reach the conclusions of their most difficult demonstrations had led me to imagine that all things, to the knowledge of which man is competent, are mutually connected in the same way, and that there is nothing so far removed from us as to be beyond our reach, or so hidden that we cannot discover it, provided only that we abstain from accepting the false for the true, and always preserve in our thoughts the order necessary for the deduction of one truth from another."

Aristotle was almost entirely concerned with establishing what had been conceived already or of refuting error, but not with solving the problem of the discovery of truth. Now and then, in reading his organon, one feels that he has almost sensed the nature of this problem, only to find that he lapses immediately into a discussion of the logic of demonstration. He thinks of confirming truth rather than of finding it.

The Renaissance gave birth to the demand for a new organon, "a scientific method which shall face the facts of experience and justify itself by its achievement

in the reduction of them to control." Bacon called for the overthrow of the dominant system, for a new beginning, for recourse to nature, for induction in a safeguarded form, for experiment, for a logic of discovery. He objected to the syllogism as constraining assent where we want a control over things. But he did not succeed in his great object of founding a logic of discovery. "It has been pointed out, and with perfect justice, that science in its progress has not followed the Baconian method, that no one discovery can be pointed to which can be definitely ascribed to the use of his rules." Descartes' doctrine of clarity as the supreme criterion for a method of discovery does not come to grips with the real problem. His extension of the method of mathematics into a general method of reasoning and discovery is not adequate to the varied needs of the investigator.

So goes the story through the whole history of logic. The developed systematic logic is a logic of demonstration. Whewell saw that "science advances only in so far as the mind of the inquirer is able to suggest organizing ideas whereby our observations and experiments are colligated into intelligible system": but he could give no direction for the capture of these organizing ideas. In the article on induction in the *Encyclopaedia Britannica* we read: "The most important faculty in scientific inquiry is the faculty of suggesting new and valuable hypotheses. But no one has ever given any explanation how the hypotheses arise in the mind: we attribute it to 'genius,' which, of course, is no explanation at all. The logic of discovery, in the higher sense of the term, simply has no existence. Another important but neglected province of the subject is the relation of scientific induction to the inductions of everyday life. There are some who think that a study of this relation would quite transform the accepted view of induction. Consider such a piece of reasoning as may be heard any day in a court of justice, a detective who explains how in his opinion a certain burglary was effected. . . . . . What the detective does is to reconstruct a particular crime; he evolves no general principle. Such reasoning is used by every man in every hour of his life; by it we understand what people are doing around us, and what is the meaning of the sense-impressions we receive."

§2. *Difficulty of a Logic of Discovery.* Two distinct causes may contribute to the failure to produce a logic of discovery, one having to do with the nature o the mind and the other with the assumed nature of th logic.

It is conceivable, as we have said, that the primary acts of discovery should be so largely or so thoroughly creative in their character that no science of inference from the known to the unknown can be developed; or if it can be developed in part, that it cannot be adequate except in a very restricted range. If the process is a creative one then it would probably be agreed that i is a process whose movements cannot be predicted o analyzed into cause and effect, so that the ascription o the results solely to creative action would have the effect of closing inquiry into the nature of the process. On the other hand, if the process is truly creative in its nature it is clear that we come to no better understanding of it by shutting our eyes to the fact. But if there

exists at all such a method of procedure lying beyond the reach of systematic analysis, it appears to belong to the greater minds and to their rarer moments. In large part the method of discovery seems to belong to a logic of discovery. Leaving unsettled the question as to whether any discovery is truly creative in character, one may justly proceed to ascertain to what extent the methods of discovery can be described in terms of a science of inference. If in the nature of things we are kept away from the goal of an adequate logic of discovery, we shall nevertheless in this way get as near to it as is possible for us. We shall, however, not fail to remember the fact that we are investigating only one of two conceivable methods of discovery.

It is evident that Bacon conceived of the logic of discovery as a unit which is scarcely separable into parts. Descartes obviously held the same view in a different form. Such seems to have been the opinion of most of those who have sought to develop the subject. In several places, I have met the term logic of discovery but seldom or never the notion of logics of discovery. It is conceivable that the logic of discovery is not one in the sense of something indivisible, but that it is relative to the field of investigation or the point of view so that one should not speak of a logic of discovery in any absolute sense, but only of such a logic as relative to a given discipline or a given goal of investigation.

The usual failure to divide the problem into the parts thus suggested has, I believe, been a chief hindrance to the development of the logic of discovery. The fact that the logic of demonstration is a unit, being the same whatever the field of investigation, has led to a too ready acceptance of the view that a logic of discovery should also be a unit.

Discovery itself may be relative to the point of view of the investigator. Through the changes induced in the philosophy of science by recent advances in physics the concept of explanation has undergone a considerable modification through the formation of a new list of basic fundamental elements in terms of which explanation has to be made. Of this change Rougier (*"Philosophy and the New Physics,"* pp. 146-147), has written as follows:

"In former days a physical phenomenon was explained by reducing it to the principles of classical mechanics, by giving to its laws the form impressed by Lagrange on the equations of dynamics. To explain phenomenon today is to give it a statistical explanatio by regarding it as the resultant of a very large numbe of underlying phenomena governed by the laws o chance. . . .

"Thus not only do the most fundamental categorie of our mind, those of space, of time, of causality, pas through an evolution with the progress of science bu the same holds even for the concept of intelligibility To explain a phenomenon is, for primitive man, t interpret it anthropomorphically by a supernatura agent endowed with psychological life in his own image; for a scholastic it is to explain it by ultimate causes; for Maxwell it is to deduce it from the principles o mechanics; for Gibbs and Boltzmann it is to accoun for it by the calculus of probabilities, by starting from system of elements subject to given conditions. Huma

reason is not *'une et entière en chacun'* as Descartes taught. It varies with the abstract or concrete nature of our thought, and in proportion as, on contact with experimental facts, the adaptation of our mind to nature becomes progressively realized."

Now when one modifies the meaning of such a fundamental thing as intelligibility, or explanation, he changes his point of view so radically that he will, in his investigations, look for quite different things from those for which he would otherwise look. There is a great difference between the way of work of one who expects to find the inner secrets of phenomena and that of one who supposes that he is only to get some convenient shorthand way of expressing the relations of phenomena without any approach to their ultimate explanation. One can set himself to find only those sorts of explanation which he deems to be possible. He may find other things by accident as it were. But he cannot seek them systematically. His logic of discovery, the way he infers in fact as opposed to the way in which he infers perfectly, varies profoundly with changes in his point of view and especially in his view as to what constitutes explanation.

When one conceives a definite law of progress from lower to higher methods of thought, as Comte did in connection with his law of the three states, he will carefully direct his own thought towards what he conceives to be the higher. Indeed, in a case so well marked as that of Comte, he will avoid entirely the methods which are conceived to be of the lower sort and will undertake to carry forward his investigation solely by means of what he conceives to be the higher method.

In this way an abstract ideal of excellence, when formed in accordance with a classification of method as more primitive or more secure, necessarily dominates the order of procedure in demonstration. The logic of discovery is a function of the ideal of excellence in different sorts of truth; it depends on the point of view.

With the conception of scientific explanation which is prevalent in our times it would be quite impossible for one to proceed as Descartes proceeded. One of his editors says of him: "Refusing to let himself be hindered by lack of adequate information, he thought out what the constitution of the world and man must be if they were to be clearly understood." Descartes conceived of clearness of thought as a criterion for truth and was convinced that God had arranged things so that true knowledge is possible. Then if it were true that the ideal of clearness of thought could be attained under only one conception of the nature of God and the world and man, then this must be the valid conception of these things. Out of the mind itself and the ideal of true thinking which was imposed upon it, he believed that some of the most momentous conclusions of science could be deduced without any experimental evidence. The necessary type of scientific explanation could be deduced by considerations having to do with the mind itself. He realized that experience is necessary for details; but the fundamental terms in which the explanation must be made he deduced by means of his ideal of clearness of thought.

The logic of discovery which is implicit in this type of argument makes no strong appeal to scientific thinkers of our day. It depends upon a conception of nature

and thought too far removed from what is now current. But the example serves to enforce the fact that such a logic may be relative to the ideal elements in the point of view.

§3. *The Locus of the Essential Step in Discovery.* Before one can proceed to a detailed development of any logic of discovery it is important that he shall determine in what part of the thinking process is to be found the essential step of discovery. It is clear that it is not in the proof of a proposition once conceived, even though with uncertainty; the latter requires only the use of the logic of demonstration, whether the conjectured proposition is established or shown to be false. The essential step is in a much more original act. It is in the formation of the conjecture itself or goes back even farther to the formation of the hypothesis out of which comes the proposition to be tested, whether by experiment or by reasoning. It [illegible] even be found in a more remote place in the process of discovery than this, its chief element resting in a principle partaking somewhat of a metaphysical nature (as in the general principle of relativity), or in an ideal of a purely abstract character (as in Descartes' doctrine of clarity). It is as if the mind were seeking to impose itself upon nature, insisting that whatever explanations we may finally adopt they shall be such as satisfy the requirements of a norm set up by the mind itself. Certain of these demands may be impossible of realization. One then constructs a norm of a modified sort. The essential step in discovery is in the construction of definite hypothesis in the form of a particular or a general law or proposition and in the formulation of a principle or

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Galileo informs us that he discovered by reason the law of distance for falling bodies and that he afterwards verified it by experiment. The Copernican assertion of the motion of the earth is neither a deduction of the pure reason nor a datum of experience but an hypothesis which has been verified. Kepler can tell us the precise date on which he conceived correctly the relation of periodic times in planetary motion though it was more than a month afterwards before he succeeded in verifying the law by detailed computations. The law of gravitation offered itself clearly to Newton's thought in 1666, but was temporarily discarded from lack of agreement with recorded observations, to be revived and accepted later when more accurate observations were available for a better check. He was so agitated over the possibility that these new observations would verify his theory that he got one of his friends to undertake the necessary computations for him because in his emotional excitement he did not feel capable of doing it himself. The laws of nature, in the absence of sufficient experimental evidence to prove them, are often conceived through a happy combination of thoughts in the mind of the investigator. Innumerable useless combinations are passed over and the vital ones rise to consciousness to bring new truth to light.

An instructive failure to realize the importance of new hypothesis in physical science is brought out by the following paragraph taken from the "Register" of one of our leading American universities where it appeared regularly for a decade and a half overlapping the end of the last century and the beginning of this:

"While it is never safe to affirm that the future of Physical Science has no marvels in store even more marvelous than those of the past, it seems probable that most of the grand underlying principles have been firmly established and that furthter advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice. It is here that the science of measurement shows its importance—where quantitative results are more to be desired than qualitative work. An eminent physicist has remarked that the future truths of Physical Science are to be looked for in the sixth place of decimals."

This conception of the state of physical science seems to have had considerable currency in the earlier nineties. Since then a veritable revolution has taken place. New theories have sprung up and have manifested remarkable vitality. An eminent physicist has advised the young men to try all sorts of "fool experiments" on the ground that there is no way to anticipate what remarkable things may thus be brought to light. After a period in which successful hypotheses were seldom formed there has come one when new and even startling hypotheses have followed one after another with bewildering rapidity, and physical science has taken such a leap forward as has been witnessed only two or three times in its history.

This emphasizes the importance of the place of hypothesis in the process of discovery.

The logic of demonstration is by definition only that sort of logic which compels assent to the conclusion when the premises are granted. It is this logic by which one is always to establish those results which are to be made secure in virtue of their logical dependence on results which are already known to be secure. It is the universal method of the mathematician when he sets forth for others the proofs of the truths discovered by him. In the natural sciences it is often true that one must start from principles which are only probably, or even only conjecturally true. There is always the possibility that some new phenomena will be brought to light not in agreement with the principles already accepted, so that one never establishes precise results with compelling logic. There is always the need for experimental test. In a certain part of mathematics this is not so, namely in that part in which the doctrine advanced gives rise admittedly to a body of results that follow from given postulates which are accepted.

This marked difference between mathematics and physical science is not altogether so universal as has sometimes been supposed. In some fields, as in that of the theory of numbers for instance, we are dealing with a set of objects which we assume ourselves to know so thoroughly that our basic propositons are not so much postulates as the statement of known properties, as of the positive integers in the field mentioned. It is conceivable that as a matter of fact we do not know the positive integers well enough for this; and that, on the basis of the initially accepted properties, we may be led to some result not holding for all positive integers. We should then be compelled to modify our statement regarding our theorems and say

merely that they are true for those entities which satisfy our basic propositions. We have such confidence, through the result of previous experience, that such a breakdown is not going to ensue that we proceed without any systematic experimental verification of our results. We do, however, subject them often to the test of more or less random numerical verification; and this is in many respects similar to the experimental test in the laboratory of some conclusion in natural science obtained from theoretical considerations.

If it is objected that we do not develop the theory of positive integers from our clear conception of their basic properties but from an assumed basis of postulates, the answer is that the latter is indeed the theoretically satisfying form, but that investigators and expositors in the theory of numbers have for the most part proceeded in the way we have indicated from propositions the truth of which they have granted without question, and have not thought of their work as giving the consequences of certain postulates so much as yielding veritable properties of clearly perceived existent entities. If it is objected that this is not a perfect procedure, it may be said in reply that it is the procedure which has actually been employed. The theory of numbers, as a matter of historical fact, has been developed from certain propositions concerning numbers the truth of which one seems to ascertain immediately from his acquaintance with integers either through experience of them or through the invention or creation of them by the human mind.

These basic truths are closely analogous to the laws of the physicist. Perhaps one has a right to accept them with greater confidence than is legitimate for the physicist in his more complex domain; but the ground of the confidence seems to be essentially of the same sort, the increased confidence being due to our fuller knowledge of positive integers than of electrons for instance. Whether this fuller initial knowledge is due to the fact (if it is a fact) that the human race created integers or is due to a longer and more intimate experimental acquaintance with integers does not seem to alter the essential character of this initial knowledge. If we accept the principles of an exact logic and agree to a given set of basic propositions, then we necessarily accept the results which flow from these propositions by means of these logical principles. Thus we are accustomed to begin the development of the theory of positive integers, not from a body of assumptions but from a body of propositions which we agree are true of positive integers—for instance, the proposition that the larger number A of two positive integers can be written as a sum of two terms one of which is an integral multiple of the smaller number B and the other is zero or a positive integer less than B. These we take not so much for postulates as for true propositions from which we begin our argument.

There are two possibilities concerning the character of this knowledge. Either we have it by an immediate insight or intuition of its truth; or we have attained it on some sort of experimental basis. If it is by the former, then we have no suitable means of knowing the validity of our insight; if it is by the latter, then we cannot be said to have tested the matter fully until we have examined every aspect. But to examine every aspect of

it we shall have to verify every logical consequence of the originally accepted propositions. Then we can never be said to know fully the truth of a proposition which we have derived logically unless we subject it to some sort of experimental test, provided that that truth is not merely one which asserts the logical connection of the propositions.

Hence, either from the lack of complete certainty of our insight or of the full reach of our experience we are in the position of being short of absolute logical certainty even for our propositions about positive integers. But we have so frequently verified our results in the past that we have attained to an emphatic confidence that they will be verified in the future. Our experimental evidence is great enough to give us a strong feeling of security. And yet it may be observed that workers in number theory still seem to feel a certain satisfaction in exhibiting numerical verifications of their more abstruse theorems.

This seems to me more like the experimental verification of the natural scientist than is usually supposed. The mathematician does not feel so keenly the need of it as the physicist; but is not this confidence, after all, due primarily to the mathematician's previous experience of almost constant success whereas the physicist has more often reached wrong conclusions, due presumably to the greater intrinsic difficulty of his subject matter? Even the physicist, as we have already seen, has passed through stages in which he was almost absolutely confident of his principles and was looking around only to find means to get the right figure in the sixth decimal place. In biology there appears to be almost the same feeling of absolute certainty that things have come to their present state through some process of evolution which is not merely one cycle in an unending sequence of repeated cycles.

In the progress of knowledge we are concerned both with the logic of demonstration for the firm establishment of truths once suspected and with the way or means by which one may come in the first place to formulate a proposition and to suspect its truth. The question also arises as to whether a sure process of inference from the known to the unknown exists—that is, whether there are well defined characteristic processes, imbued with full logical rigor, by which one may pass directly from the known to the unknown in such a way that in the very passage to the new truth there is inherent the forcible logical demonstration of that truth. Or, should we seek rather some sort of heuristic logic by which one comes first to formulate a proposition whose truth he suspects, while the demonstration of it is to be sought later by more secure processes?

It seems certain that the former alternative is not realized. There is no secure logic of discovery different from the logic of demonstration. Whatever process of reasoning ends with a new truth, demonstrated as it is attained, is carried out only by a secure logic of demonstration. This does not mean that the latter is never a logic of discovery; in fact, it is often this. Many truths which assert merely the logical dependence of propositions are attained by a logic of demonstration—especially when the propositions are conceived in their abstract form. And not a few others are also derived in this way. Maxwell's prediction of the pressure of

light resulted from a truth discovered by a logic which carried with it a demonstration of the fact that this truth is a consequence of accepted laws of physical phenomena. The most striking recent instance of this sort of discovery is that of the bending of a ray of light in a strong gravitational field, as predicted by Einstein in his general theory of relativity.

§4. *Character of the Logics of Discovery.* It appears to me that the use of a logic of demonstration for the purposes of discovery does not afford a typical instance of the logic of discovery. If there is any point to considering the latter at all it is because it has, in important instances at least, characteristic qualities which are worthy of investigation. Accordingly we turn now to a further consideration of the question of the existence of some sort of heuristic logic by which one comes first to formulate a proposition whose truth he suspects, while the demonstration of it is to be sought later by more secure processes. That such processes of inference exist has certainly been recognized since the time of Aristotle. Analogical reasoning is of just this sort and so is the conclusion from the particular to the general. But the problem which we have in mind is not so much that of the general principles of probable but insecure inference, as of that which arises in consideration of some such question as the following:

Have the particular sciences certain heuristic logics and do these vary, in whole or in part, as we pass from one particular science to another? This question forces upon our attention another, namely, the question as to whether all logic is one or whether logic is relative to the field or the subject matter to which it is to be applied The foregoing separation of logic into two parts seem important here; and we should probably press a two-fol question: Is the logic of secure demonstration one the same in all ranges to which it may be applied, or i it something relative to the subject matter under in vestigation? Is the logic of discovery, the guiding bu nevertheless not absolutely trustworthy logic of th preliminary stages of an investigation, the same fo all ranges of subject matter or is it relative to th subject matter of the different sciences? Without at tempting to go into a full discussion of the questio we may say that the secure logic of demonstratio appears to us to be one and the same whatever the fiel of investigation. The forms of reasoning which in on science compel assent to its conclusions from accepte propositions are the same as those which in any othe science have the same compelling power. On form, for instance, mathematical induction, may b rather frequently employed in one science and appea seldom or never in another; but it is valid whereve it applies and has the same compelling power.

But it seems not improbable that a certain heurist logic in one science may have no conceivable place all in another.

In some investigations which I have carried o in the past few years, I have had occasion to treat great variety of related transcendental problems b means of methods to which I was led by certain fund mental algebraic guides to transcendental problem Such guides appear to have been first employed Sturm (in 1836) and by Cauchy (see Moigno's le

tures, 1844). They were brought into great prominence in more modern times through the initiative of Volterra whose work in this direction first became explicit in his publications in 1896. They appear to be of such a nature as to be useful only in mathematics; and in fact in only a certain well-defined region of mathematics, though their full value here seems not yet to have been realized in accomplished use. It is of interest to note the sort of results to which they give rise. One is led by them to a more definite and precise formulation of a variety of problems originating from certain applications of mathematics to physical phenomena, the formulation being so sharp and clear as to enable the mind to concentrate its thought upon the leading issues and to avoid the waste due to a distraction of attention by irrelevant matters. The central fundamental theorems around which the detailed theory of these problems gravitates are suggested so clearly by the heuristic process to which one is led in an unmistakable manner as to leave no room for a failure to discover these theorems, at least in a wide range of problems. The process does not directly and immediately afford us a proof of the theorems. But it does yield precise suggestions concerning the method of proof by which one may establish them through a rigorous logical procedure. This particular heuristic logic,[1] then, serves the three-fold purpose of making the problems definite, of suggesting the central theorems, and of indicating suitable methods of proof.

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[1] This heuristic logic afforded the principal subject of my retiring address as Chairman of the Chicago Section of the American Mathematical Society in December, 1921; a detailed account of it for mathematicians will be found in that address as published in the *Bulletin* of the Society for April-May, 1922, pp. 179-210.

Perhaps this example represents the extreme of definiteness and serviceability in these heuristic logics. All gradations exist between this and that other in which the inference from the known to the unknown is through well-defined processes which are imbued with full logical rigor and by which one may pass directly from the known to the unknown in such a way that in the very passage to the new truth there is inherent the compelling logical demonstration of that truth. Let us for a moment contrast this latter sort of logic of discovery with the former.

We can get it before us best by taking an example where it would naturally be employed. Let us suppose that one has observed that the positive integers may be separated into two classes: in the one class are those positive integers each of which is a product of two smaller positive integers; in the other are all positive integers not in the first class. Let us call the integers of this second class prime numbers. Let us suppose now that one has already found out in some way that every integer of the first class contains as a factor some prime number greater than unity. Suppose then that he raises the question as to the number of integers in each class. In both classes together there is an infinitude of numbers, since these classes together contain all positive integers. That the first class contains an infinite number of integers is obvious, since it contains an infinitude of powers of each integer or since it contains the double of every positive integer. The question which remains and calls for answer is whether the number of primes is infinite. The answer, complete or in part, must evidently be one or the other of the following: the number of primes is finite; the number of primes is infinite. The order of procedure is obviously to assume one or the other of these alternatives and to test it; if we assume the wrong one we can expect to arrive at a contradiction. Let us try out first the simpler assertion that the number of primes is finite. Then let P be the product of all of them, and consider the number P+1. It is divisible by no one of the primes except unity, since we have a remainder of one on any such division owing to the fact that P is now supposed to be the product of all prime numbers. Hence we have a number larger than any prime and without a prime factor, in contradiction with what we already knew. Hence, the number of primes is infinite. Our question is therefore answered with a precision which may be accepted as tentatively satisfying.

Here the process by which we arrive at the answer to our question contains the proof that the answer is correct. Here the logic of discovery is in no wise different from the logic of demonstration.

But there is something peculiar about this case which is not present in all cases. The question whose answer we sought has by its nature one of a finite number of answers which press themselves at once upon the attention as the logical possibilities, and this almost as soon as one has clearly conceived the question. Let us ask, on the other hand, what is the law of force among the atoms or parts of atoms in chemical combination. There is no such finite set of exhaustive and useful logical possibilities to arrest the attention. So far as the logical elements in the situation are involved there is an infinitude of logical possibilities of co-ordinate importance. What we know about the matter is far too little to compel as inference one or the other of any finite set of useful logical possibilities. We cannot proceed to the desired truth by means of logical processes compelling the conclusion and demanding confidence in the results attained. We need some logic of discovery different from that which is suitable in demonstration.

Such a heuristic logic will be necessary partly (and roughly) in proportion to the definiteness and completeness of the underlying truths already in hand and on which we proceed to build the theory. Such necessity will increase with the complexity of the problem to be investigated and the consequent difficulty of an orderly procedure from the known to the unknown. Hence there are two stages in the development of a science when one will be in an especial need of a heuristic logic. The first of these is that necessary for a science in the nascent stage of its development when its underlying basic principles are being discovered and put in order. This need will subsist in greater or less measure for each experimental science as a whole, and especially in its infancy. The second of these necessities is that which arises in the remote and complex developments of some phase of a science when one wishes to branch off rather widely from the beaten trail and to develop a new chapter or section of the science.

It is not our purpose to consider the logic of discovery outside of the domain of the exact sciences nor indeed to discuss the variety of heuristic logics suited to

the various sciences or their several parts except in so far as this may be convenient in analyzing the general character of such logics. From the example which we have exhibited from mathematics it must be clear that a logic of discovery may be special to a particular well-limited class of closely related problems and hardly have a point of contact with any other investigations whatever. Other examples with the same character can be found in mathematics, especially in those fields where the physical intuition can be brought to bear upon the mathematical problem, as in the theory of differential equations (ordinary and partial) with boundary conditions. It seems likely that this relativity of the logic of discovery to the particular subject matter of investigation will be found to be a characteristic of it in all divisions of science.

§5. *Danger of Error.* The tentative nature of the logic of discovery allows room for an error of a dangerous sort. In some cases the measure of sagacity required in the successful use of such a logic is not very great, and the investigator is therefore able to sense accurately a considerable class of results without a need for discarding any. His success for a time is so uniform that he begins to lose sight of the tentative character of his processes of inference and to think of these as secure in the sense that they guarantee the validity of the conclusions attained. Then he gradually ceases to feel the need of a test of verification and is inclined to be satisfied without it, particularly if it is hard to devise such a test. He begins to have an undue confidence in his heuristic method. He has often found it successful. If it has ever led him astray he has seen clearly where he lacked in sagacity. In the new situation he seems to have avoided all extraneous sources o error. He concludes therefore that the result heuristically attained is valid even though he has not tested i independently.

It appears that an error of this sort is especiall likely to arise in those domains of natural science i which one initially makes large abstraction of the actua complexity of the phenomena in order to bring the within the range of successful investigation.

Let us take an example of this, purposely put int extreme form in order to make the point clear.

Let us suppose that one is investigating the processes of thought. He examines all the observable circumstances connected with a process which yields a poe or an hypothesis in natural science or the consequenc of some physical law or a theorem in mathematic Waiving the question, for the moment, as to whethe there is something hidden which he cannot see, he proceeds to make a complete catalog of all that he can fin as he looks upon the thinker while in the process o thought. He varies the individual under investigatio and the circumstances under which he is examined an the subject matter of his mediations. After a time h has a large number of facts of observation. Testin them for common properties, he finds that every one o them is of the nature of a physico-chemical fact. The are the sort of thing which the careless observer migh call the physico-chemical concomitants of the processe of thought. Our investigator is too careful to nam them in this way; for that would already be to rea into the observed facts a large measure of deep-lyin

hypothesis, and it is desirable to avoid this lest we read our prejudices into the facts. The subject whose processes of thought are being investigated will not be asked to give an account of his own experience; for, in doing so, his prepossessions will necessarily color his account. The matter must be made more thoroughly objective than would be possible under such a plan of procedure. The observed facts are before the investigator, shorn of everything which might be colored with prepossessions. They are all physico-chemical in their character. In how far may they account for the processes of thought? For each recorded movement of the thought process there is a physico-chemical phenomenon. What is the connection between them? In how far can one describe or explain the processes of thought in terms of these physico-chemical changes? These are natural and legitimate questions.

Let us suppose that a very considerable success has been attained in setting up a definite one-to-one correspondence between isolated items of thought and particular physico-chemical changes, so that one may measure certain movements of the physical frame and tell the subject truly at least a part of what he was thinking at the moment. As the work proceeds the investigator becomes more successful in recording the thoughts by means of the observed reactions of the physical frame. He begins to raise definitely the question as to how far he may go in accounting for thought in terms of physico-chemical changes and (let us say) he begins to incline to the view that a complete success is possible. In his meditations he begins to say to himself that if there is anything in the thought process besides these physico-chemical changes then that part of it cannot be subjected to scientific investigation. A long experience with the matter inclines him more and more to identify these physico-chemical changes with the thought processes which he started out to investigate; and he is led finally to assert the identification.

This case I have purposely made extreme; but I find it difficult to tell how far our psychologists have gone in this direction. Some of them have gone quite far enough to leave me bewildered. They seem to have laid aside some of the fundamental elements in the problem and to have neglected the fact that they have done so. Or have they merely delimited the field of "psychology" and left the study of the mind, in the older sense, to philosophy or to some science not yet created?

If I seem to have departed from my subject of the logic of discovery I wish now to come back to it and to say that it is legitimate to make any tentative abstraction of elements whatever that may seem desirable in a particular investigation, provided that it is always remembered that such abstraction has been made. It is desirable to know in how far the processes of thought can be described and explained in physico-chemical terms. But it is undersirable to allow the success in establishing the correspondences between the two things to obscure the fact that the style of the investigation necessarily leaves out of account entirely a certain type of phenomenon, and that it therefore throws no light on the question of the existence or non-existence of this type of phenomenon.

It has been said that "the greatest discovery ever made in philosophy was that the way to discover whether a thing is present is to look and see." In ancient times the exponent of this doctrine was Aristotle, while Francis Bacon brought it to clear notice in the modern world long after Telesio and Roger Bacon had unsuccessfully insisted upon it. But, as applied to external nature, the doctrine was not in good repute with Plato who considered it erroneous and upheld as true that which agreed with his sentiments of propriety and beauty. Since the latter were supposed to have been ascertained by looking inward upon the mind, this process of introspection finally came into disrepute because it led to contradictions with what was found by looking upon the external world to see. This disrepute of the practice of looking inward has been so great that some psychologists seem to have become afraid to use in their science as a method of discovery the simple one of looking and seeing. Their logic of discovery, so some of them insist, must be one of induction from the observation of physico-chemical phenomena. To an outsider they seem to be in need of finding out again that the way to discover whether a thing is present is to look and see, at least in matters pertaining to the experience of concentrated thought. Their logic of discovery seems to have been made too narrow.

This narrowing of the range of the logic of discovery is not peculiar to any one science. It seems to me to be an ever-present danger in the necessary form of the process of discovery in any natural science. We cannot deal at once with the whole complexity of phenomena. We choose a certain part of them and try to find our explanations in terms of that part. It is always admitted that complete explanations have not been found; but that failure is accounted for by an insidious error common in what I would like to name the proof by ignorance. It is likely to arise when great abstraction has been made and this fact has been ignored. A good example of it in a general situation is afforded in condensed form by the following quotation:

"If, then, it is impossible, through deduction, beginning from the transformist hypothesis, to build a theory of morphological evolution verified by experience, this is not because the hypothesis is false; it is because it is incomplete and corresponds solely to certain factors of evolution. In order to foresee the results it is necessary to know all the factors. The apparent indetermination arises solely from the insufficiency of our knowledge."

"The apparent indetermination arises solely from the insufficiency of our knowledge." Here in a single sentence is the essence of the proof by ignorance which in one form or another is often advanced in scientific discussions and sometimes treated in a dogmatic way widely variant from the spirit of true science. If one is to maintain the method at all he will probably have to do it merely by dogmatic assertion; for there does not appear to be any argument in its favor. It should be apparent to everyone, on reflection, that we can never prove any positive proposition, any significant truth, by means of our ignorance of the facts in the case—except the one fact of our ignorance. When it is said in the foregoing statement that "the apparent indetermination arises solely from the insufficiency of our knowledge" the truth of this cannot be known from

our knowledge of the facts, for we are confessedly ignorant of them. Three possibilities arise then: either the truth of the statement is not known at all; or it is known by some transcendental insight into external phenomena; or it is known through our ignorance of the facts. There is no room to doubt what the scientific conclusion is in the matter: the statement is not known to be true. It is neither demonstrated logically nor verified experimentally.

Perhaps we should dwell still longer upon the absurdity of this proof by ignorance, for it contains the essence of frequent error which vitiates many conclusions. W. K. Brooks, in his *Foundations of Zoology,* truly says: "The hardest of intellectual virtues is philosophic doubt, and the mental vice to which we are most prone is our tendency to believe that lack of evidence for an opinion is a reason for believing something else." In the absence of any evidence that the indetermination is not due to our ignorance we are inclined to conclude that that is the ground of it. The second law of thermodynamics rests on just such insecure foundations. We know no terrestrial facts to dispute it; we are far from having established it. The present situation warrants our taking it as an hypothesis to see what we can get out of it; but it does not justify any uneasiness of mind as to what it may say about the future history of the universe. The principles of the conservation of energy and of the conservation of mass, or their modern combination into a single principle, may well serve as a working hypothesis. But we must remember that no conclusion based on an hypothesis of such a character carries with it its own validity. Such a logic of discovery requires to be supplemented by an independent of the result, and the latter may be accepted only provisionally in the absence of such a test.

§6. *Ideals as to the Form of Truth.* There is something finer in a possible logic of discovery than anything which we have so far made explicit. One may arrive at truth not only by induction and deduction but also under the impulse to realize directly an ideal as to the form of the truth to be attained. The most striking recent instance of this is found in the general theory of relativity as developed by A. Einstein. In order to bring out clearly its character in this case we shall have to present certain elementary considerations associated with one aspect of the theory of relativity.[2]

Let us approach the matter by thinking of a geometrical curve fixed in the space interior to a given room of four walls meeting at right angles. If we take the floor and two adjacent walls to be a system of reference by means of which to locate the positions of points in the room, then we can uniquely define the position of a point on our curve by giving its distance from the floor and from each of the two walls selected. If the point moves along the given curve then the numbers expressing these distances will vary and will be related according to a law determined by the shape and position of the curve; these three variable numbers will satisfy certain equations of condition. Now suppose that we modify our procedure by using the ceiling and the other

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[2] By taking the technical mathematical terms in their usual non-technical sense the non-mathematical reader will have a sufficiently clear idea to make the argument intelligible.

two walls as a system of reference. Since the relation of the curve to this system of reference is in general different from that to the former we obtain in general different equations of conditions for the same curve. These conditions for the same curve would be modified still further if we should choose some other set of three mutually perpendicular planes for the system of reference, and especially so if these planes should be oriented in some new directions.

It is clear that the properties of the curve itself have in no wise been affected by these changes in the system of reference, even though we have several times modified the mathematical expressions by means of which these properties may be most compactly and most completely described. Let us for a moment forget these systems of reference and consider the curve itself by passing along it from point to point. Two characteristics will force themselves upon our attention: The amount of bending of the curve as we pass along it, its curvature; the amount of twisting of the curve, its torsion. These are intrinsic properties of the curve itself, capable of representation at each point on it by definite numerical values. These numerical values can be expressed in terms of the three distances pertaining to any given one of the systems of reference mentioned above; it turns out that definite rather simple formulae exist for expressing the curvature and torsion in terms of the named measurements. Since these describe intrinsic properties of the curve their values must be unaltered by the transformations of variables due to the changes in the system of reference; that is, they must be invariants of the transformation.

Thus it is seen that the analytic expressions for the curvature and torsion are unchanged in form and in value as we pass from one of our systems of reference to another. It can be shown that they completely determine the intrinsic properties of the curve. Then we have in them a complete mathematical description of the intrinsic properties of the curve in a form from which we have abstracted those peculiarities which belong to the special system of reference by means of which we described the curve and its position in the first place. This sort of abstraction is of frequent and important use in mathematical investigations. It affords one of our methods of excluding from consideration those things which are irrelevant to the central purpose of the investigation and of fixing attention upon those things alone which are unaltered by, or are invariant under, the transformations permissible among the elements in consideration. A similar but extended use of invariants gives substance to the ideal which guided the development of the general theory of relativity.

Two considerable extensions are necessary before we can realize precisely the situation in the development of the Einstein theory. The first has to do with a generalization of the system of reference. We must replace the three mutually perpendicular planes of our system of reference by three warped surfaces, perhaps twisted and corrugated and irregular in shape and restricted only enough to allow us to utilize them successfully for the unique location of points in space. By means of these we are to describe the space configurations with which we have to deal. The other extension

consists of the introduction of time into our system. We cannot well develop the mechanics of three dimensions by means of what is merely geometric in three dimensions; but if we introduce time and think of our space-time continuum as affording a four-dimensional world, then our mechanics in three dimensions is replaced by a geometry in four dimensions. In the Einstein theory one no longer tries to maintain the separation of measured space and time; they are not independent; they are indissolubly united into a four-fold space-time extension. In this space-time of four dimensions we are to choose as a system of reference four warped three-dimensional spaces by means of which the location of points in this four-dimensional space-time shall be defined. We can go from one such system of reference to another by a change of fundamental variables. The totality of these changes is said to form a group, and certain subsets of them are called subgroups.

With these conceptions in mind, it is easy to make clear the nature of the ideal upon which Einstein insists as to the character of the laws of nature. He wishes to have them expressed in such form with respect to this four-dimensional continuum that there shall be no change in the form of these laws when we pass from one of these systems of reference to another; the mathematical relations expressing the laws are to be invariant when all quantities involved are changed in accordance with a transformation from one system of reference to another: let us say for convenience that the laws are to be stated in covariant form. When we have put them into such form we have abstracted

from the statement of them everything which pertai
to the particular system of reference employed.

It is a grave question whether the laws of natu
are capable of formulation in accordance with the r
quirements of such an ideal; and an affirmative answ
can be maintained only after a searching examinatio
All precise evidence which exists up to the present tim
is in favor of the conclusion that such an ideal may b
realized.

It is not necessary to our present purpose to go int
a further analysis of the question as to whether th
ideal may be realized in practice. We wish to loo
upon it as affording an example of a logic of discover
One here sets up a certain ideal as to the form of th
laws of nature. He then takes those known law
which agree closely with experimental facts and e
quires whether their statement meets this ideal. Th
affords the best way to make trial of the validity
the character of the law which this ideal would impo
upon nature. If the law as previously conceived me
this ideal there is a certain satisfaction, but there
nothing further to be done with this law. The inves
gator passes to another in order to discover, if pos
ble, one which is not yet subject to this ideal. Su
pose that he finds one, as Einstein did in the case
the Newtonian law of gravitation. It turned out th
this law does not accord with the ideal of covaria
of the laws of nature. Shall one then give up the id
on the ground that the Newtonian law is so well est
lished that any deviation from it required by a n
ideal shows that this ideal is not realizable? No,
on this evidence alone; it may be after all, that the l

of Newton is not exact and that some modification of it will bring it into covariant form without disturbing its agreement with observed facts. If so, there is likely to be some range of facts, perhaps not previously observed, in which the two laws will give measurably different results; and one will then have a crucial experiment by means of which to discard one in favor of the other.

As a matter of fact, the Einstein theory appears to have triumphed over the Newtonian theory in precisely this way. Whether it has or not is not essential to our present purpose. We are concerned with the heuristic logic involved in the process. In accordance with the Einstein demand it is desirable to enforce upon nature, if possible, a certain ideal as to the mathematical form of the statement of the laws of nature. This ideal guides one's investigations and leads to conclusions as to laws of experimental phenomena. It does not prove these laws; for we have not yet any means of knowing that the laws of nature are capable of expression in covariant form. But it does give us certain new laws, or modified forms of old laws, which we would probably not have reached except under the guidance of such an ideal. The ideal thus gives rise to a logic of discovery. The supposed law once attained is subjected to a searching test. If it survives under this test, then we have a veritable advance brought about by the guidance of an ideal which affords a heuristic means of inferring the unknown from the previously known.

According to this aesthetically satisfying ideal of Einstein, then, we are to have in the mathematical form of the laws of nature a complete covariance under the general group of transformations of coordinates in the four-dimensional space-time continuum. But in the complete realization of this ideal there are certain difficulties of the nature of mathematical complication the avoidance of which would be welcome. The question, then, arises naturally as to whether we might not take certain subgroups of the general group of transformations and apply these to the separate fields for the purpose of obtaining approximate laws—laws which are covariant under the subgroup applicable to a given field or under several subgroups of a given type. Perhaps one may not have as strong expectancy of the validity of such a law as of one which is wholly covariant; nevertheless one may naturally expect in this way to make closer approximations than he would make without this aid.

This is a new sort of approximation in theoretical physics of a much more profound character than numerical approximation. It is an approximation in the sense demanded by covariance under a subgroup instead of under the whole group. As the subgroup is enlarged, and the statement of the law undergoes consequent modification, the approximation will presumably become closer and may even become as close as is needed for agreement with experiment even though the entire group of transformations is not employed.

The group of the Lorentz-Einstein transformations of the special theory of relativity is precisely such a subgroup as we have just described; and it is well known how it has led to more satisfactory theories of certain phenomena than those which had preceded them. Under this group the Maxwell-Hertz electrodynamic equations and the wave equation deduced from them are invariant. Now, if all the phenomena in a certain field are invariant under this transformation group then every mathematical formulation of law in this field should be in the form of an equation which is covariant under this group. This would afford a precious guide as to the necessary form of such an equation. If an equation is obtained in an empirical way, then one considers it only as a rough approximation to the true equation; and one seeks a better form of it which shall have the two properties of being invariant under the named group and of being represented to a suitable approximation by the empirical equation first obtained.

It seems not unlikely that each large and well-defined class of phenomena may have associated with it a certain group of transformations—a subgroup of the general group of transformations of axes in the four-dimensional space-time continuum—of such sort that the phenomena in question (or at least the most of them) may be represented to an approximation quite within the range of experimental error by requiring merely that modification of approximate empirical laws which is required to bring them into a form that shall be covariant under the given subgroup of the total Einstein group.

This procedure might well have the advantage that it may be carried out much more readily than the corresponding one based on the original group, at least if the sub-group is of simple character, say analogous to the group of all multiplicative linear homogeneous transformations.

From certain mathematical considerations it seems probable that in many important cases one would actually obtain in this way precise laws the covariance of whose statement under the general Einstein group could be readily established. The number of invariants relative to a given subgroup and agreeing with a given rough empirical law to a suitable approximation may often be small or be even unity. In the latter case it must be the law which is covariant under the general Einstein group, if there is such a law. In the former case, one would probably have little difficulty in choosing the appropriate one for general covariance (if such a one exists).

Thus it seems likely that a systematic study of the covariance of laws under subgroups of the general Einstein group will lead to useful means of discovering the laws of phenomena. Thus for each of these subgroups we appear to have the possibility of a logic of discovery of the same general sort as that employed by Einstein in his general theory and differing from the latter only by virtue of its being relative to a subgroup of the Ein stein group instead of to the whole group. Thus i seems probable that we may have in physical scienc a considerable variety of logics of discovery based o transformation groups as the ultimate ground under th ideal as to the form of statement of the laws. O course, every law obtained in this way (as well as i any other) must be subjected to experimental test be fore it can be accepted. The method would profess onl to help in discovery. Some of the results to which would lead would turn out to be valid (if the gener theory of relativity is valid) and others would nee

further modification under the guidance of a more comprehensive group.

§7. *Summary.* It is conceivable that in the nature of things the mind is unable to comprehend its highest movements with clearness and also that certain of its acts of discovery are essentially creative in character; but neither of these possibilities need affect the development of a logic of discovery in the sense of a logic by which one infers from the known to that unknown which hitherto has not been apprehended or suspected. Such a logic may lead forward by well-defined steps to clearly ascertainable propositions without carrying with it a demonstration of those propositions. The already developed systematic logic is a logic of demonstration. The logic of discovery, in the higher sense of the term, has no existence as an actually developed science. This was insisted upon in effect by Descartes and Francis Bacon and remains as true in our day as in theirs. Bacon's supreme effort to found a logic of discovery ended in failure. No single discovery can be pointed out which can be definitely ascribed to the use of his rules.

There are two causes which may have contributed to this failure. It is conceivable that the primary acts of discovery are so essentially creative in their character that no science of inference from the known to the unknown can be developed; or, if it can be developed in part, that it cannot be adequate except in a very restricted range. This does not seem to have been the main difficulty. The conception of the logic of discovery as a unit, comparable in unitary character to the logic of demonstration and scarcely separable into parts, has, I believe, been a chief hindrance to its development. We can have not so much a logic of discovery as logics of discovery each relative to som field or subject matter of investigation. Discovery i relative to the point of view. We can no longer proceed as Descartes did when he "thought out what th constitution of the world and man must be if they wer to be clearly understood."

The essential step of discovery is in the formatio of a conjectured proposition to be tested or of an hypothesis out of which the proposition comes or of an ide which guides in the formation of both hypothesis an proposition. It is possible that truth may be discovere by means of a logic which carries with it ~~in~~ the demonstration of the truth; but the more characteristic logic of discovery are merely suggestive in character. Th former is often illustrated in mathematics, particular in the theory of numbers and the theory of finite group In physical science it is the latter which is generally i evidence. This latter also has a wide usefulness i mathematics. Every result of a heuristic logic must b subjected to some suitable adequate test after it is obtained. In mathematics it is often demonstrated by *de novo* argument. Such a heuristic logic serves th three-fold purpose of making the problems definite, o suggesting the central theorems of an investigation, a of indicating suitable methods of proof.

The tentative nature of logics of discovery allo room for an error of a dangerous sort. When th measure of sagacity required in the use of such a log is not great, one may so uniformly succeed with it for time as to lose his sense of the need of an independe

test of the result. By hidden gradations of error one may then pass step by step to the condition of being satisfied with the unsound "proof by ignorance" so that he is able to conclude an argument with the absurd climax: "The apparent indetermination arises solely from the insufficiency of our knowledge."

The finest thing associated with a logic of discovery is that one may arrive at truth under the impulse to realize directly an ideal of far-reaching importance as to the form of the truth to be attained. The most recent striking instance of this is to be found in the general theory of relativity. In accordance with Einstein's demand it is desirable to enforce upon nature, if possible, a certain ideal as to the mathematical form of the statement of the laws of nature. This ideal guides investigation and leads to new laws. It affords a heuristic logic associated with a general group of transformations in space of four dimensions. It seems probable that certain related but more special logics of discovery of distinct usefulness may be associated similarly with certain subgroups of this general group and that we may thus have in physical science a considerable variety of logics of discovery each based on a transformation group as the ultimate substance of a related ideal as to the form of statement of the laws.

CHAPTER II

WHAT IS THE PLACE OF POSTULATE SYSTEMS IN THE FURTHER PROGRESS OF THOUGHT?

§1. *The Nature of Postulates.* It is not so much my purpose to answer this question as to give more general currency among thinkers to the practice of asking it. It seems clear that postulate systems have been employed fully and consciously in only a small part of that domain of thought in which their use will yield characteristic values that perhaps cannot be realized without them. Too much of what is known on the subject is now known to a relatively small group of thinkers, whereas the ideas involved are of such sort as to deserve wide currency and to be useful to thinkers in widely separated fields of thought. Thence arises my desire to give them greater currency. Certain main features of the question have been recently discussed by J. Rueff in an illuminating way in his book, *De Sciences physiques aux Sciences morales,* published by Félix Alcan, Paris, in 1922. This book will be of value to those who deal with the so-called exact sciences and of great value to those who are concerned with the ethical and the economic sciences.

Postulates were formerly known as axioms; and axioms were held to be self-evident fundamental truths. In this sense they appeared in Euclid's *Elements.* The

attitude toward them was profoundly affected by the non-euclidean geometries; there has been a progressive change in the judgment concerning their significance; and today we see them in a light very different from that which illuminated them for the ancients. We shall not attempt to trace the stages of this development. For our purposes it is sufficient to have in mind a clear conception of what is meant today by a postulate system. Every body of logically connected doctrine must have a starting-point from which the deductions may proceed. This basic material must contain propositions which are taken as assumptions, that is, are not proved. Such propositions are postulates. A fruitful connected consistent body of such propositions constitutes a postulate system. They afford the basis from which deduction may begin. So far as deduction is concerned they are entirely unproved. In the natural sciences there is sometimes direct or indirect empirical evidence for their truth; and in mathematics there is an esthetic guide to their formation so that in some cases they may be far removed from experience with the external world. In all cases considerations of comfort to the thinker himself enter into their formation and enunciation.

Not only must we have unproved propositions in a deductive body of logically connected doctrine; we must also have undefined terms. These terms are substantive or relational. A substantive term is one pointing to an existent thing which may be either material or mental, as for instance the term *point* in certain postulate systems for geometry. A relational term names a relation holding for two or more terms; as, for example, the relation of betweenness when one point is said to lie between two other points. While such terms are undefined in the sense that no explicit definition is or can be given to them in the system in which they appear they are not unrestricted in meaning. They appear in the fundamental propositions of the postulate system; hence in every concrete instance of the doctrine or "doctrinal function", built on these propositions the undefined terms must have such significance as to verify, or render "true", the basic propositions in which they appear. Hence, while they are not and cannot be defined explicitly, they are nevertheless such as to verify the postulate system. Sometimes this may leave a great range of variation open to them; at other times it may restrict them in a comprehensive way, as in the case of those postulate systems which are called categorical. An interesting discussion of this whole matter in a form accessible to the "educated layman" is to be found in Lectures I to IX of C. J. Keyser's *Mathematical philosophy,* published by Dutton, New York, 1922.

Typical examples of postulate systems are to be found in mathematics. Those which occur in geometry are perhaps the ones most readily accessible to the layman. It is in mathematics that the notion of postulate system has been most carefully developed and utilized most consciously and most widely. Fortunately the ideas connected with the whole treatment of postulate systems require for their understanding no more knowledge of mathematics than is usually the possession of a college-trained person. To appreciate them requires a certain intellectual maturity and acumen, but very little technical mathematics. That they are thus

accessible to the educated layman is made abundantly clear in Keyser's *Mathematical Philosophy.* It seems that the way is now open for postulate systems to take their rightful place in the further progress of exact thought in all fields of intellectual activity.

§2. *The Situaton in the Natural Sciences.* The natural sciences also contain their postulate systems in much the same sense as mathematics contains them, though perhaps not in such a central role and certainly not as consciously. In order to see clearly their place in the natural sciences it is necessary to make a sharp distinction between two parts of those sciences. One part is empirical; in this the basic fundamental laws are secured by induction and subjected to the verification of a preliminary test as to their validity. Such a process as this must continue until the science has reached a stage of considerable advancement. Then it enters upon a second phase of its existence in which an attempt is made to set apart a few of the empirical laws as basic and fundamental and to derive all the remaining empirical laws from these by a strict deduction. Furthermore, the logical consequences of the fundamental laws often point to the existence of new empirical laws not yet recognized. If these predictions are not verified some modification of the basic fundamental laws will be necessary; they are often corrected and extended in this way. On the other hand, these predictions attained through logical deduction are often verified by experience; then we have a real advance in our knowledge of phenomena. The basic fundamental laws in such a scheme of logical deduction play a role analogous to that of a postulate system in a mathematical doctrine, the likeness becoming greater as the basic laws become more conventionalized. Rational mechanics may thus be expounded on a strictly postulational basis. The same is true to a less degree of other parts of physical science, and particularly of the theory of electricity and magnetism. The expositor of physical science, more or less consciously, puts his exposition into a form similar to that of a doctrinal function based on a postulate system in mathematics.

But the matter in physical science does not rest here. This could be brought out well by an appeal to the current theory of relativity. But one need not go into so recent and so narrowly understood a subject to make the point. What we have said about rational mechanics may be put much more strongly and thus be made to serve the purpose. If one reflects how poorly defined are the terms matter, energy, force, one may begin to see that the so-called laws of mechanics are not so much laws about clearly defined entities as they are postulates serving to define the range of use of fundamental terms not otherwise strictly delimited in meaning. In fact, a good deal could be said for the thesis that rational mechanics consists of a body of doctrine derived from propositions of essentially the same character as the postulates of mathematics. From this point of view the basic propositions of physical science are not so much the statements of empirical laws as of the postulates from which the deductions proceed. They justify their use if they lead logically to empirical laws and to no conclusions which are empirically invalid. Thus the law of the conservation of energy is one which by its nature is incapable of direct empirical

validation; our ground for believing it is that it leads logically to empirical laws.

There can be no doubt then that some of the presuppositions of physical science are of the nature of postulates in mathematics. This has not always been realized. It is insisted upon successfully by Rueff in the book cited. We emphasize again the desirability of proceeding consciously from this point of view and of seeking those primitive ideas and those primitive propositions from which empirical laws may be logically deduced. This is a method of attack upon the problem of scientific explanation which has not been systematically exploited; in fact, its use has often been implicit and unconscious even in the hands of those who may be shown to have employed it.

§3. *The Nature of the Problem.* Several preliminary questions will come immediately into the foreground of attention if one proceeds deliberately to build up science in this way. First of all perhaps will come the question as to whether there are some postulates which must or may underlie all thought. Two of these will come at once to mind: the principle of non-contradiction in the realm of thought itself; the principle of cause and effect in the realm of phenomena, either in those terms or in others, the terms and the meaning depending largely upon one's philosophical prejudices. Certain general considerations of this sort will pertain to all thought or to all thought about phenomena. It is a function of logic to analyze these questions underlying all thought. Important as the matter is we shall pass it over now in order to give our attention specifically to the more immediate subject matter of the present chapter.

But there is one general question of procedure so intimate to our present problem that we cannot pass it over without some analysis. Let us suppose that one is seeking the primitive ideas and the primitive propositions from which a given body of empirical laws may be logically deduced. However extensive this body of laws may be, it can never restrict the investigator to a unique set of primitive ideas and primitive propositions. He has always a large measure of choice to be determined by considerations of esthetic satisfactions or of convenience. This is well brought out by the experience of the geometers. In the case of euclidean geometry, for instance, various systems of postulates have actually been set up as a basis for the one body of doctrine. For a given body of doctrine we may have numerous postulate systems; for a single postulate system we may also have a great variety of concrete bodies of doctrine.

If we apply this to the physical sciences we shall see that we have a certain large measure of freedom in our choice of primitive ideas and primitive propositions. This has been so marked in some cases as to lead to the conclusion that man has in some way actually imposed his mind upon the phenomena of the external world. Some enjoy the contemplation of this conclusion; others are not moved by the empty pride to which it gives rise.

But this partial freedom of choice of primitive ideas and primitive propositions can serve a more mundane and tangible end than this. It makes it possible to raise

hopefully some such question as the following: In the case of several related bodies of empirical laws, as, for instance, those of the different divisions of physics, is it possible so to choose the primitive ideas and primitive propositions of each of them that the several systems will have a common part—this part being clearly non-categorical? Leaving aside for the moment the consequences of a positive answer, let us observe that the experience of physics has shown that just such a thing is possible. Witness the way in which rational mechanics, and hence its postulate system, underlies several divisions of physical science.

So far as I am aware, there has never been a systematic analysis by any one of the possible forms of postulate systems such that the systems for several related bodies of empirical laws would have a common part. A little attention to the matter, however, will show the great value of such an organization from at least two points of view, namely, that of economy of thought and that of an intimate understanding of the deep-lying connections among the phenomena. The latter value must be obvious at once; the former will become so if one reflects that it must then be possible to develop once for all the consequences of the common body of primitive ideas and propositions and to apply these directly to each of the related bodies of empirical laws.

It seems to me that the time has now come to proceed more boldly and more consciously in the way of developing science by means of postulate systems deliberately chosen with reference to their effectiveness in leading to known empirical laws. Such a procedure is sure to meet with opposition in certain quarters; it will be called artificial; some will desire to have the basic propositions so chosen as to be at the same time basic fundamental laws, and they will lose sight of the fact that science in the form to which they are accustomed and with which they have been satisfied is far from proceeding always in the manner which they desire. The fact that science has customarily employed such artificial hypotheses, or rather fictions, for various important uses has been obscured by the order of historical development of these hypotheses. Vaihinger has rendered to science a great service of clarification by insisting upon this in his philosophy of the "As If" (*Die Philosophie des Als Ob*). Light moves *as if* it were propagated by an ether having certain properties; and scientists have for a long time employed the conception of an ether even though they never had any direct experimental evidence for its existence and are now inclining to discard it altogether. They employed it as a fiction rather than as an hypothesis, in the stricter sense of the latter term. These fictions (as opposed to hypotheses) have played a leading part in the scientific, philosophic, religious, and even in the artistic life of man (so Vaihinger argues); and our plea now is for a more conscious use of them in connection with the construction of postulate systems to give deductive form to all bodies of exact thought.

§4. *Essential Character of the Process.* Let us consider the essential character of the process of putting science into a deductive form based on postulate systems consisting of primitive propositions concerning primitive ideas. These primitive ideas and propositions will be chosen solely with reference to the appropriate deduction of the empirical laws from them by means of logical processes. If it is more convenient to do this by means of fictions belonging to the philosophy of the "As If" rather than by means of hypotheses in the stricter sense, one shall not hesitate to employ the former; in fact one will feel that there is a certain gain in having a more lively sense of the fact that it is this which we are doing rather than in proceeding more or less unconsciously as we have often done heretofore. We shall seek simply to form a system of postulates from which the laws may be deduced. As we proceed to derive the consequences of these postulates we are certain to have three types of experience. Sometimes we shall deduce known empirical laws as the consequences of our hypotheses; this will be the usual experience, since our postulates were set up primarily for this purpose. But we shall sometimes also deduce propositions pointing to the existence of empirical laws which we have not yet observed. Our experience with these propositions will be of two sorts. Sometimes we shall find by subsequent observation that the laws predicted by our propositions are veritable laws of phenomena; then we shall have gained a real advance. But sometimes again we shall find that our deduced propositions point to laws which do not have an empirical existence. This will be a sign to us that something is wrong with our postulate system. And we shall therefore have to proceed to the task of revising or modifying that system so that it shall realize the known laws and avoid this consequent proposition which we now know to be false. It may be that we shall have to reconstruct our postulate system entirely. This will not mean that our previous labor is lost, for the earlier organization into a postulate system and consequences will facilitate the newer organization which must be undertaken. In this way we shall have a constant interaction between our postulational treatment and the empirical laws with a side by side development of the two. It is much in this way that science has advanced. We are urging that it shall do so more consciously and more systematically.

Rueff, in the book cited, has laid the foundations of economics on this basis; and he has done so in an illuminating way which will be instructive for workers in other fields of thought as well as in his own. By means of certain ideas and propositions, artificial enough in themselves, he shows how known laws of economics may be deduced. He is not concerned so much with validating the basic hypotheses as he is with finding suitable ones to yield the necessary conclusions. To validate the hypotheses as corresponding to actuality we should have to show that no others can yield the required conclusions. It seems highly improbable that we shall ever succeed at this for economics or any other science; and certainly we shall not so long as the science in question is incomplete in any essential part. But Rueff is not concerned with the problem of validating his hypotheses as being required by the empirical laws: he wants merely some suitable system sufficient to yield the laws; that there may also be other systems does not interfere with the success of the one which he employs. If there is a better one than his own that one

is to be preferred; but it is a fundamental gain to have a single one that works.

Such a method of expounding and advancing truth may invade the ethical sciences also. Probably some of its chief conquests are to be made in this field of thought, as Rueff has pointed out. Let us definitely set ourselves the problem of finding the most satisfactory hypotheses, postulates, from which we may deduce logically the accepted empirical laws of conduct. These hypotheses, or postulates, are then to be subjected to test and to be modified in the way we have already indicated in the general case. From them we shall seek to deduce whatever conclusions they may engender. When we arrive at a known empirical law we shall assign to it its proper place in the exposition. When we get a new one which observation verifies we shall rejoice in our conquest. When we obtain a proposition which is not verified by experience we shall seek the needful modification in our system of postulates. Thus our science of ethics shall grow and develop. In our hands it will become a rational ethics in the sense in which mechanics is rational, leading to a fuller knowledge of the laws of conduct and a deeper understanding of their nature.

A method which has proved of so great use wherever it has been employed should be thoroughly utilized in every domain of thought which has become exact enough to allow its use. We do not urge its employment to the exclusion of other methods. It cannot do everything: no method can. But so long as the means of advancing knowledge remain uncertain in their consequences we cannot afford to allow any promising method to lie unexploited, and especially such a one as we have been considering. Shall we in the future utilize it as far as it can reach? It can be employed successfully wherever definite empirical laws can be formulated.

After a doctrine has reached a very considerable development (with postulates and theorems) it is desirable to analyze it from the point of view of replacing one or more postulates by certain of the theorems so as to have the same body of doctrine as before. This will give us alternative forms for the postulate system and so lead us to a deeper understanding of the connections of the whole body of doctrine. It will also have a special value of great importance with reference to the experimental verification of a principle of science which is still in doubt. This was well illustrated a few years ago by the special theory of relativity (a fact which is emphasized in my little book on that subject, published by Wiley and Sons, New York, 1913; second edition in 1920).

This consideration of the postulational treatment of science yields one interesting by-product to which we must briefly refer in conclusion. The geometers have found a means of developing the so-called non-euclidean geometries—geometries which seem bizarre to the unsophisticated imagination, but are nevertheless altogether logical in form and hence contain no internal contradictions. This suggests the existence of what Rueff calls non-euclidean ethics and non-euclidean economics. Such a non-euclidean ethics, for instance, would be completely free from internal contradictions and therefore could never be refuted by the finding of inconsistencies in it. The only possible means of refutation would be in the comparison of it with experience. One who propounded such a doctrine could not be vanquished within his own domain. It would be necessary to carry his conclusions to the fundamental test of experience in life.

CHAPTER III

ON THE NATURE OF SYSTEMS OF POSTULATES

§1. *Example of a System of Postulates.* Certain elements of structure are common to all deductive expositions of truth. As a particular science develops and acquires well determined laws it has a tendency to assume a deductive form. If we have before us the body of such a science, geometry for instance, with its terms and definitions and propositions, and we ask how it may be put into most satisfying logical order, we shall be brought to the conception of a system of postulates as the necessary apparatus from which the deductive procedure may begin. We must choose certain elements and relations to be left undefined, at least so far as geometry is concerned, and also certain propositions to be left unproved. These unproved propositions are called postulates. When they are agreed upon and set before us with such descriptive fullness as will lead to a clear understanding of their import we are prepared to go forward with the systematic development of the whole body of propositions which constitute the science.

All this is a commonplace of current mathematical thought. To a greater or less extent these truths have been realized since the days of Euclid (who called by the name of axioms what we now call postulates).

Their import and bearing have been discussed by many writers. The main facts of a general nature, which it is necessary for the reader to have in mind in order to understand the present discussion, have frequently been set forth, and nowhere to better advantage than in Lectures II to IX of Keyser's *Mathematical Philosophy.* All such expositions, so far as I know them, satisfy also certain additional demands not essential to our present purpose. Accordingly, there is more fullness in them than we need now, so that it seems best for us to give just such an account of postulate systems as will enable the reader to understand our later argument.

If we are careful, at the outset, to guard against a certain possible error, we may employ for our purposes an exceedingly simple system of postulates—one so simple, in fact, as to risk the danger of appearing to be trivial. This is the error against which the reader must be warned. Though very simple in itself, this system of postulates may serve for us the purpose of exhibiting in a concrete way (and with the least complication) certain fundamental properties of postulate sets in general. It has already been employed, for a purpose somewhat similar to ours, in Veblen and Young's *Projective Geometry.* Moreover, it is akin to the more comprehensive system of postulates on which these authors base a remarkable development of projective geometry. A rather full philosophic discussion of Hilbert's postulates for geometry is given by Keyser in the lectures referred to. If one has in mind the fact that postulate systems have served such important purposes in geometry as those already indicated, not to mention their uses elsewhere, he will not be misled by the exceedingly simple (and perhaps apparently almost trivial) character of the example which we are about to give as a concrete definition of the meaning of a system of postulates—an idea which must be clearly in mind if one is to understand what we shall have to say in the next chapter about the postulational treatment of empirical truth in general.

Certain logical notions are necessary in the formation of any set of postulates. That of class is one of the most central. Closely connected with this is that of "elements of a class" and the relation of "belonging to a class." For our purpose, we shall take these as primitive notions, into the meaning of which we do not inquire. We may then set up our system of postulates thus (following Veblen and Young, l .c.):

Let S be a class the elements of which are denoted by A, B, C, ... We shall need certain undefined sub-classes of S (classes consisting of a part only of the elements of S), any one sub-class of which we shall call an m-class—thus employing for the undefined subclass a term which is meaningless in itself. Concerning the elements and m-classes of S, we now make the following assumptions:

I. If A and B are distinct elements of S, there is at least one m-class containing both A and B.

II. If A and B are distinct elements of S, there is not more than one m-class containing both A and B.

III. Any two m-classes have at least one element of S in common.

IV. There exists at least one m-class.

V. Every m-class contains at least three elements of S.

VI. Not all elements of S belong to the same m-class.

VII. No m-class contains more than three elements of S.

In this system of postulates (or assumptions), we have two undefined terms, namely, element of S, and m-class of S; we have also one undefined relation, namely, belonging to a class. So far as the miniature mathematical doctrine based on this system of postulates is concerned, these terms are entirely devoid of any specific content except in so far as that content is implied by the postulates themselves. We shall see later that the postulates do, as a matter of fact, very definitely limit the meaning of these terms though they do not make that meaning unique.

Concerning any such system the primary question is that of the consistency of the postulates contained in it. Are there mutual contradictions in the statements contained in the postulates? If so, the system can probably be of no important use; if not, how shall we be assured of that fact? How may we be certain that in deriving the consequences of these postulates we shall never be led to two propositions which directly contradict each other? It is not enough that the propositions which we actually derive exhibit no contradictions. We wish to be assured that it is impossible that the consequences of the postulates can in any way involve a contradiction.

Now the only known test of the consistency of a system of postulates is one which shifts the difficulty from one domain to another. We shall see how this fact is illustrated by the discussion of the consistency of the foregoing set. Let us consider the letters A, B, C, D, E, F, G, each taken three times, all of them being written in an array of three rows and seven columns, as follows:

A B C D E F G
B C D E F G A
D E F G A B C

Now, let us think of these seven letters as affording a concrete instance of the elements of S contemplated in the system of postulates; and let us look upon the elements in each column of the foregoing array as constituting an m-class of S. We ask whether each of the seven postulates is true when the undefined terms are given these specific meanings. Taking the postulates one after another we find that each of them is verified by the concrete instance afforded by this array; this the reader may ascertain for himself by a consideration of each postulate separately.

Consider what this result signifies. Since each postulate is satisfied for the given array of elements, every proposition which is deduced solely from the given system of postulates, by strictly logical processes, must be true of this array of elements. Now this array of elements does not possess contradictory properties. Hence, no contradictory propositions can result from the given system of postulates. Thus its consistency is established.

If one examines carefully into this argument, he will see that it depends in the last resort on a certain

assurance of self-evidence in the verification of the postulates by the array of letters. We seem to ourselves to see by a direct insight (or intuition) that the postulates are true if taken with the indicated concrete meaning; and our conviction of their consistency depends on our confidence in this direct insight. Such, in the last resort, is a characteristic of all so-called proofs of consistency.

To prevent misconception, we should say here that the given concrete instance of elements and m-classes of S is not the only one which verifies the given postulates. To this point we shall return in a later section.

In the next three sections we shall give some detail of a mathematical sort. This presupposes a certain intellectual maturity on the part of the reader and the willingness to give consecutive attention to the discussion; but no technical mathematics beyond that of the high school is necessary for its understanding. The results of the discussion and the principal notions involved contain all that is essential to what follows. It will be sufficient if the reader acquires the general ideas and notes the principal results. He may dispense with proofs entirely, if he prefers to do so. The details are given for those who wish to go into them and to make it possible for any reader to verify the conclusions stated. The more essential matters are so emphasized that a rapid reading cannot fail to bring them to the attention of a selective mind.

§2. *Consequences of the Foregoing System of Postulates.* For illustrative purposes, we shall now prove certain theorems which are implied by the postulates given in the preceding section. (The reader who desires to do so may dispense with the proofs, reading only the theorems.) Some of the theorems are such immediate consequences of the postulates as to require no further arguments, while others are of a different nature. At the end of each theorem we shall indicate, by numbers in parenthesis, what postulates were employed in the proof of the theorem. In this discussion we shall have to ask the reader's indulgence for the introduction of considerable detail; the results are necessary for use in the analysis of the principal matters to be treated later.

1. If A and B are distinct elements of S, there is one and only one m-class containing both A and B; it may be called the m-class AB (I, II).

2. Any two distinct m-classes have one and only one element of S in common (II, III).

3. There exist three elements of S which are not all in the same m-class (IV, V, VI).

4. Each m-class contains just three elements of S (V, VII).

Let A, B, C be three elements of S which are not in the same m-class. The m-class AB contains a third element D so that it consists of the three elements, A, B, D. The m-class BC is different from the m-class AB and hence contains an element E, which is different from A, B, C, D, so that BC contains the elements B, C, E. Now consider the m-class AE; it cannot contain either B or D, since it would then coincide with AB; nor can it contain C, since it would then coincide with BC; therefore, AE contains a new element F, so that AE consists of the elements A, E, F. Next consider the m-class DE; it cannot contain either A or B, since it would then coincide with AB; nor can it contain F, since it would then coincide with AE; nor can it contain C, since it would then coincide with BC; therefore DE contains a new element G, so that it consists of the elements D, E, G.

Let us now examine the m-class defined by each pair of the seven elements, A, B, C, D, E, F, G; in each of the twenty-one possible cases we shall find that the third element is one of this set of seven elements. The m-class ABD accounts for the pairs, AB, AD, BD; the m-class AEF for the pairs AE, AF, EF; the m-class BCE for the pairs BC, BE, CE; the m-class DEG for the pairs DE, DG, EG; there remain nine other pairs. Now BF and DEG must have an element in common (by postulate III); since it cannot be D or E, it must be G; the m-class BFG accounts for the pairs BF, BG, FG. Again, AG and BCE must have a common element; since this cannot be B or E it must be C; the m-class ACG accounts for the pairs AC, AG, CG. Again DF and BCE must have a common element; since this cannot be B or E it must be C; the m-class CDF accounts for the pairs CD, CF, DF. Thus we know the elements in each m-class determined by a pair of the seven elements, A, B, C, D, E, F, G; and there are just seven of these m-classes.

Let us now consider the possibility of an eighth element H in S. If such an element exists then AH and BFG must have a common element. If this were B then ABDH would belong to the same m-class; if this were F then AEFH would belong to the same m-class; if this were G then ACGH would belong to the same m-class. Each possibility contradicts postulate VII by requiring at least four elements in an m-class. Hence S does not contain an element H different from all the elements A, B, C, D, E, F, G.

Thus we have the following theorem:

5. The class S contains just seven distinct elements, A, B, C, D, E, F, G; and these occur in just seven distinct m-classes according to the array given in the first section (I-VII).

Moreover, from an inspection of this arrangement of the elements of S in m-classes—now known to be necessary—we have the following theorems:

6. There is no element of S which is in all the m-classes of S (I-VII).

7. If a and b are any two distinct m-classes of S there is one and only one element of S in both a and b (I-VII).

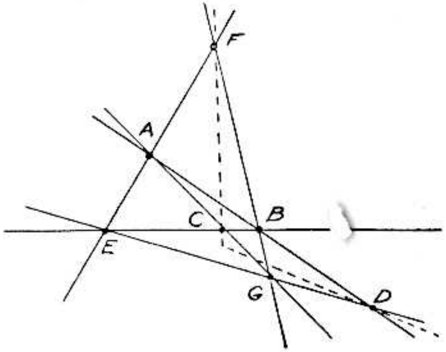
8. Every element of S is in just three m-classes of S (I-VII).

§3. *Interpretations of Systems of Postulates.* In section 1 we gave in the form of an array of letters A, B, C, D, E, F, G, each taken three times, one concrete instance of elements and m-classes of S. We may say that these afford a concrete interpretation of the system of postulates in section 1. In section 2 we saw that in the abstract general case the elements of S which satisfy these postulates are seven in number and that they may be represented by the letters A to G disposed in the same manner as in the concrete instance previously considered.

Let us now examine a second concrete interpretation of the system of postulates. Consider the adjoined figure. Seven points are indicated by the letters A

to G and these lie by threes on seven lines (the six straight lines and the dotted line FCD). If these seven points are taken as the elements of S and the three on any one of these lines as an m-class of elements of S, it is easy to verify that postulates I to VII are all satisfied by such an interpretation.

Let us exhibit still another concrete interpretation of the system of postulates. Let us take for the elements of S the seven triples (100), (010), (001), (110), (101), (011), (111). Let us define an m-class



$[u_1\ u_2\ u_3]$ to be those triples $(x_1\ x_2\ x_3)$ of the foregoing set each of which makes the number $u_1 x_1 + u_2 x_2 + u_3 x_3$ even, where $[u_1\ u_2\ u_3]$ denotes one of the seven symbols [001], [100], [110], [111], [011], [101] [010]. Then the elements of these m-classes are in order those in the following seven columns:

(100)	(010)	(001)	(110)	(011)	(111)	(101)
(010)	(001)	(110)	(011)	(111)	(101)	(100)
(110)	(011)	(111)	(101)	(100)	(010)	(001)

It is easy to verify directly that these elements and m-classes of S satisfy all the postulates of section 1. If the elements in the first row of the foregoing array are denoted by the letters A to G in order then the array becomes identical with that of the letters A to G in section 1.

These three concrete interpretations of the system of postulates are very different in character. The first consists of a set of tags or labels denoted by the letters A to G and certain combinations of them; the second is geometric; the third is arithmetic. The theorems given in section 2 apply to each of them. Notwithstanding their difference in the concrete they obviously have much in common in the abstract. From the theorems in section 2 it follows that any two interpretations of the system of postulates must have a certain marked and precise mutual correspondence between their elements. In fact, if S_1 and S_2 are two classes S subject to the system of postulates, every element of S_1 may be made to correspond to a unique element of S_2 in such a way that every element of S_2 is the correspondent of a unique element of S_1, and that to every m-class of S_1 there corresponds similarly a unique m-class of S_2. The two classes S_1 and S_2 are then said to be in one-to-one reciprocal correspondence or to be simply isomorphic. When a system of postulates is such that every concrete interpretation of it is thus simply isomorphic with every other concrete interpretation of it the system is said to be categorical. The the set of seven postulates in section 1 is categorical.

But if the last postulate is omitted the remaining six form a system which is not categorical. For, the

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concrete interpretations of postulates I to VII form such interpretations of postulates I to VI, while these latter are also verified by the 13 elements A, B, . . . , M and the m-classes denoted by the following thirteen columns:

A	B	C	D	E	F	G	H	I	J	K	L	M
B	C	D	E	F	G	H	I	J	K	L	M	A
D	E	F	G	H	I	J	K	L	M	A	B	C
J	K	L	M	A	B	C	D	E	F	G	H	I

§4. *Properties of Systems of Postulates.* Two of the fundamental properties of postulate systems we have already treated incidentally to the earlier discussion, namely, consistency and categoricalness. Nothing more need be said concerning consistency. But we wish to add something concerning categoricalness. We have already seen that a system of postulates may be categorical or that it may fail to be categorical but we have not analyzed the ways in which it may fail to be categorical. If we consider a set of postulates which is not categorical and examine all the concrete interpretations of that set we shall find that these concrete interpretations themselves fall into two classes such that those interpretations which are in any one of the classes have the mutual property of being simply isomorphic. We may think of the various concrete interpretations in a single one of these classes as being equivalent in the sense of being simply isomorphic. The number of these classes of equivalent interpretations may be said to measure the multiplicity of the non-equivalent interpretations of the system of postulates. The question arises as to whether this multiplicity may be infinite and whether it may be equal any (whatever) given finite number k. The answer affirmative for each case. It may be shown that o postulates I to VI, taken alone, are of infinite mult plicity, while postulates I to VI and the following po tulate

VII_1. No m-class contains more than $n+2$ el ments of S where n is a given positive integer

afford a system of multiplicity k if n is properly chosen We shall not take the space necessary to prove thes statements. We shall merely add that the k possibili ties in the latter case arise from the fact that an m-clas may have any one of k distinct numbers of element thus giving the k distinct possibilities.

The question arises as to whether it is preferabl that a system of postulates shall be categorical or non categorical. The answer depends upon circumstance There is often a certain advantage in keeping the se non-categorical as long as possible. Let us make thi concrete by means of an example. If we should de velop the consequences of our postulates I to VI, with out the use of any other postulate whatever, we shoul have a body of theorems which are true for any co sistent set of seven postulates obtained by adjoinin another to these six. That other might be the following

VII_2. Every m-class contains just $n+2$ elements o S where n is a given positive integer.

By varying n over a suitable set of positive integers w have an infinite number of distinct possibilities for th seventh postulate. The consequences of postulates I t

VI are true for each of this infinitude of cases. Thus, by starting with a non-categorical set of postulates we are able to develop a common part of a variety of doctrines. It is easy to see how this may result in a great economy of thought. In the next section we shall discuss the implications of this important principle for the development of natural science.

Let us next consider the question of the independence of a set of postulates. If no one of the set can be derived as a logical consequence of the others the set is said to be independent. For our original set of postulates I to VII we have already seen that VII is independent of the others; for the array at the end of section 3 affords a concrete interpretation of I to VI which does not verify VII. Again, if we take the elements of S to be A, B, C, and the m-classes of S to be the pairs AB, BC, CA, then postulates I, II, III, IV, VI, VII are verified while V is not verified. Therefore V is independent of the other postulates of the set. In like manner, other examples could be constructed to show that each of the postulates I to VII is independent of the other postulates in the set. On the other hand, if we should adjoin to these seven postulates an eighth one reading like theorem 7 of section 2, then we should have a set which is not independent. For some purposes it is not necessary, or even desirable, that the set of postulates should be independent. The bearing of this on the development of empirical truth will be discussed in the next section.

Two other properties of a single system of postulates may be mentioned briefly. It should be fertile, that is, it should have consequences which are not explicit (but are only implicit) in the postulates themselves. It should be compendent; that is, it should not be separable into two or more unrelated systems of postulates.

If we have two systems of postulates such that those in each system may be derived logically as consequences of those in the other system, we say that the two systems of postulates are equivalent. If we consider the body of propositions in a given doctrine—the postulates and their consequences—as a whole, it is evident that two equivalent systems of postulates give rise to the same body of propositions; it is for this reason that the term equivalent is applied to them.

In section 2 we have already derived certain important consequences of the postulates in section 1. We shall now restate seven of them in a form to serve our purpose for the study of equivalence of systems of postulates, following them by numbers in parentheses referring to the theorems which contain them.

Ia. If a and b are distinct m-classes of S there is at least one element of S in both a and b (7).

IIa. If a and b are distinct m-classes of S there is not more than one element of S in both a and b (7).

IIIa. Any two elements of S are elements of some one m-class of S (1).

IVa. There exists at least one element in S (3).

Va. Every element of S belongs to at least three m-classes of S (8).

VIa. There is no element of S belonging to all the m-classes of S (5, 8).

VIIa. There is no element of S which belongs to more than three m-classes of S (8).

It may be shown that this system of seven postulates is equivalent to the system in section 1. In fact, we have already shown (in section 2) that those in this section may be derived logically from the system in section 1. That those in section 1 are consequences of the system in this section may be shown in a precisely similar manner; the argument is so much like that in section 2 that we may omit it.

This example illustrates one further important fact concerning systems of postulates. Suppose that we have before us a considerable body of propositions (we say nothing now of how they were derived), and that we wish to organize them as far as possible into deductive form. It is necessary to choose a system of postulates from which the deduction may proceed. The example before us shows that this system of postulates need not be unique; there may be more than one way to select it. In fact, a little meditation on the matter will show that there must often be a great freedom of choice for the basic postulates. This is a matter of great importance in natural science, since it allows the investigator a considerable freedom to follow his sense of esthetic fitness. We shall return to this point later.

§5. *Postulates in Mathematics and Laws in Natural Science.* The miniature mathematical science which we have (partially) developed on the basis of postulates I to VII illustrates what is done in more fullness in reducing any mathematical discipline to an abstract and logically connected form. We choose certain terms and relations, one or more of each, which are left undefined; and we select certain propositions which involve these terms and relations and are to be left unproved. These propositions constitute the system of postulates. They serve to limit and restrict the possible meanings of the undefined terms and relations. All other propositions are to be deduced from these by logical processes. The resulting science is purely abstract, is a part of pure mathematics strictly so called. It may have several concrete interpretations. If the system of postulates is categorical any two of these interpretations will be simply isomorphic. Otherwise, there will be two and perhaps many interpretations which are not simply isomorphic in pairs.

One of the principal objects of such an abstract formulation is to exhibit with precision the logical connections of the various propositions of the science. The method has great unifying power and brings out clearly the analogies among things which at first appear to be distinct. In this way it serves an important use in economizing thought. The development of one abstract doctrine may carry with it at the same time the main essentials of that of several or many concrete doctrines. Moreover, it is possible to deal for a while with a non-categorical system of postulates and so develop with a single effort the common part of several or many distinct doctrines either abstract or concrete.

With the conception of a system of postulates in mind, it is easy to make clear what is meant by rigorous thinking. By "thinking rigorously" one means that sort of process by which one obtains from a given system of postulates and from nothing else but that system some or all of the consequences which flow from that system by logical necessity. There is no implication that this is the only sort of thinking that is legitimate; but it is intended to include all sorts which proceed in a legitimate way solely from a set of postulates to necessary conclusions which they imply. In those parts of natural science which are not essentially applied mathematics, and indeed in some parts of what is often called applied mathematics, the thinking is not strictly in accordance with this ideal. There often are definite hypotheses, or postulates, furnishing a part of the necessary basis of the argument; but these are frequently supplemented with a sort of intuitional knowledge of experimental facts, a sort of incomplete induction, so that the conclusions reached have not been engendered solely by the definite and consciously realized hypotheses, but owe an essential part of their existence to the additional unstated assumptions or conclusions from experiment which constantly appear in the argument. In mathematics, strictly so named, none of these unstated or implicit postulates may be employed. Whenever they are present the thinking is not rigorous in the proper sense of the term.

It is not an easy matter to throw a comprehensive discipline into this strictly rigorous form; and yet we do not properly understand its logical connections until we have organized it in this way. It is difficult, in the theory of electricity and magnetism for instance, to select and establish experimentally those fundamental basic laws from which all the properties of electricity and magnetism may be derived by logical processes alone; and yet, so long as we have not done this, we have not brought the science to that ideal state which is so forcibly suggested as desirable by the experience of mathematicians in dealing with the simpler subject matter of their science. In some parts of natural science, as in celestial mechanics, for instance, the achieved success is so great that the theory is almost or quite capable of exposition in a strictly rigorous deductive form. To bring a science into this state requires long labor and the work of many minds. The discovery of the fundamental laws is usually not the achievement of an individual but of a long line of individuals; like many other elements of our civilization it is a racial achievement and belongs to the race. It is one of the fruits of the past, a part of the accumulated wealth from the toil of many generations of dead men fructifying the labors of the living.

The conception of the postulational treatment of a body of doctrine may afford us an important criterion for determining the state of development of a science with respect to its attainment of empirical laws. Are the known laws sufficiently definite to afford the basis for setting up a postulational treatment? Are there among them a few of such central importance that they may be made the starting-point for a rigorous logical deduction of all the others or at least of certain large bodies of them? If this is not so, then one can hardly suppose that the more central empirical laws of the science are yet discovered. In the more highly developed of the natural sciences there exist these fundamental basic laws—laws which may be taken as the postulates in a deductive exposition of a large body of propositions each of which is verified by natural phenomena. In others, and particularly in the sociological sciences, this stage of development has not yet been reached.

Suppose that one considers the body of known laws

in some particular science, say for definiteness the science of psychology. Suppose that one examines these laws with respect to the question as to which of them may be deduced rigorously from the others; and suppose that he finds, as perhaps he would find in the science of psychology in its present state of development, that there is no comprehensive group of these laws which are logical consequences of those in a small set to be taken as a system of postulates. What then will be his conclusion concerning the state of development of that science? He may say that the science in consideration is of such a nature as not to be capable of reduction to a rigorous form after the manner of a postulational treatment, and he may insist that the ideal suggested by the clear conception of a body of doctrine intimately connected logically is not a suitable ideal for this science and that this ideal is as a matter of fact foreign to the nature of this science. There is something which could be said for this point of view. But when he observes that every science which is generally reputed to be in a high state of advancement approaches more or less nearly to this ideal and that it approaches it more nearly as it becomes more highly developed, he may come to doubt after all whether his first position is correct. He may be led to conceive an ideal for the less developed science which is thus so forcibly suggested by the more highly developed. It appears to me that the known fact of the existence of important and extensive bodies of doctrine in the ideal deductive form afforded by a postulational treatment should be kept in mind by every scientific investigator as suggesting a fundamental criterion by which he may estimate the degree of advancement of his own science; and that it should often spur his determination and effort in the direction of singling out the more central and fundamental laws of his science.

It is not necessary, and perhaps not even important, that this ideal should be applied in its strictest form. So far as the advancement of empirical truth is concerned it does not seem to be necessary that the laws which are selected to constitute the system of postulates for a deductive exposition shall yield a set whose propositions are independent. If one desires to see clearly the logical connections of these laws he will wish to have a system of independent postulates. He may also desire to know several equivalent systems of postulates. But this knowledge can hardly be of much value for the advancement of empirical truth; its importance lies in the help which it gives in penetrating clearly into the logical connections involved.

But there will be a certain gain in economy of thought—and sometimes a great one—if certain important laws can be selected which are rich in consequences and at the same time afford a non-categorical system of postulates, particularly if these laws are common to several branches or divisions of science. The same thing will often be gained by the use of a categorical system with several concrete interpretations. An important example is afforded by the mathematical treatment of several branches of physical science. Let us make the matter concrete by taking an example. (It is not necessary for the reader to understand the mathematical terms: the essential point will be clear to him without this knowledge.) Consider the partial differential equation $\frac{\delta^2 u}{\delta x^2} + \frac{\delta^2 u}{\delta y^2} = 0$, where u is a function of x and y. Among the theories in which this equation is fundamental, or at least important, are the following:

A. In pure mathematics:
   In the theory of functions of a complex variable;

B. In physics:
   (1) In the flow of incompressible fluids;
   (2) In the steady flow of heat;
   (3) In the steady flow of electricity;
   (4) In the theory of static electricity;
   (5) In the theory of magnetism.

As we have said, the differential equation is important in the study of each of these widely separated topics. Abstractly considered, there is a certain common body of laws in these several disciplines which gives rise in each of them to the differential equation in consideration. From our present point of view we may look upon these laws as affording a certain system of postulates. One would probably formulate this system in such a way as to be non-categorical; whether he does or not, the example will still serve our principal purpose, namely, to see that the postulational treatment of these disciplines will give rise to great economy of thought; for, whether categorical or not, there are several concrete interpretations of the system, namely, those indicated by the several disciplines of which they form a common part. When one develops the properties of the given differential equation he obtains results which are applicable simultaneously to each of the several named disciplines. Thus a single theory will imply important consequences in each of several directions.

We have said enough to make it apparent that the postulational treatment of empirical truth must have important consequences in the development of that truth. This will be more fully apparent in several ways as we proceed with the treatment in the next chapter.

CHAPTER IV

CONCERNING THE POSTULATIONAL TREATMENT OF EMPIRICAL TRUTH

§1. *On the Nature of Scientific Investigation.* The point of departure in the development of science is the living human being. The beginnings are to be sought in the slow and almost unconscious observations of primitive men. Out of the complex of impressions due to interactions with the environment there was a gradual separation of crude facts and these isolated elements of experience became increasingly definite with the lapse of time and the multiplication of instances. After a long period arose a conception of certain uniformities, such as the apparent movements of the heavenly bodies. A mastery over rude implements was gradually attained and the primitive peoples began to have some tools by the aid of which to increase the security and comfort of their lives. These served at the same time as the means by which their crude investigations were carried forward.

In the more advanced stages of the racial history the point of departure in the development of any new science is still the living investigator. The ancient Greeks appear to have made the mistake of not getting far enough away from this beginning; they attributed too much to the thinker and gave too little place to nature as manifested in the environment, so that they did not learn to properly question her by experiment as to the laws in accordance with which she operates. Their science was too metaphysical. At the origin of modern science the pendulum swung so far in the opposite direction that the importance of the investigator himself was often lost sight of in the midst of the multiplicity of facts and laws which he encountered. But it seems clear that all science is, by its very nature, a phenomenon of the observer and the observed and that both of them play fundamental roles. The laws of man's spirit as well as the laws of other nature are wrought into the propositions of all science.

To lay the foundations of a science nowadays, it is necessary to begin with an accumulation of facts—and some of these must be the results of measurements if the science is to be exact. It is necessary to examine these facts with respect to the uniformities which appear among them. Here incomplete induction and intuition will play a fundamental role. When a given phenomenon, so far as our experience goes, follows regularly upon the appearance of certain other phenomena we conclude that it is connected with them by some sort of law, and we form an hypothesis as to what that law may be. Often we find it necessary to discard an early hypothesis in favor of one later formed in the presence of a more comprehensive body of facts. Thus, with more or less change and after a longer or shorter period of testing, our hypotheses are modified and extended and refined until they come to appear to us to be veritable laws of nature. Our science is then approaching a stage in which the investigators can begin to look upon it somewhat as an actual existent entity which they can behold as exterior to themselves.

It is at this point that a critical analysis of the developing science can be profitably made. The workers begin to see connections among the laws and to single out some of them as more fundamental than the others. There is a tendency to consider some of these more central laws as affording the causes of some or all of the remaining laws in the science, and the logical deduction of some of these from the others begins. Though some freedom in the matter may be allowed to the investigator by the nature of the phenomena, yet these causes cannot be chosen at random. If they are to play the role of postulates in a deductive exposition of the science—and this is what they have a tendency to become—they must be selected in such wise as to afford a suitable starting-point for the chain of deductions. When the science has reached a stage in which these central laws or postulates can be singled out with some completeness its exposition tends to assume a strictly deductive form. In a book on geometry or rational mechanics or theoretical physics there is little of incomplete induction or intuition in the processes of analysis, however much these may have been present in the discovery of the laws which are developed, and the procedure is dominated by a certain logical necessity in the connections of the propositions, often to the exclusion of all other processes of thought.

For our present purpose it is necessary to see clearly that these fundamental laws in a natural science play a role in it precisely analogous to that of a system of postulates in mathematics—such a one, for instance, as we have exhibited in the miniature mathematical science of which we have given a partial exposition in the previous chapter. It is true that an empirical part of the science must precede the systematic exposition in which this has place; but it is also true that the science is not in a satisfactory state of completeness until expositions of it exist in this systematic form. We need not examine now the relative importance of these two parts in the development of a science; it is sufficient to our present purpose to observe that both of them are essential.

When these fundamental laws have been set apart and the known empirical laws have been deduced from them there is left the problem of ascertaining what other empirical laws are implied by the fundamental ones already investigated; thus the exposition will itself yield predictions of new empirical laws not yet recognized. In this way it may contribute to a veritable advance of the science: as a matter of fact, it has often done so in the past.

If one reflects how poorly defined are such terms as matter, energy, force, one may begin to see that the so-called laws of mechanics are not so much laws about well-defined entities as they are postulates serving to define the range of use of fundamental terms not otherwise strictly delimited in meaning. As we have seen this is precisely one of the important functions of the system of postulates in a mathematical science. In the example which we treated in the previous chapter the terms "element of S" and "m-class of S" were entirely undefined except in so far as they were restricted by the propositions concerning them contained in the system of postulates. This did not restrict them, as we

saw, to a single concrete interpretation; but in the system to which we gave our principal attention it did so restrict them that all concrete interpretations of the postulates were simply isomorphic. Such a restriction of the meaning of the terms would be quite enough for most of the purposes of natural science. Then, if the fundamental laws are treated as postulates are treated in a mathematical science, the investigator can obviate a large part of the difficulty which he must otherwise feel with respect to the definitions of terms. From this point of view the basic propositions of a science need not be so much the statement of empirical laws as of the postulates from which the deduction of the empirical laws may proceed. They justify their use if they lead logically to the empirical laws and to no propositions which are empirically invalid. Thus the postulates need not necessarily have an empirical validation; it is sufficient if they yield the empirical laws. Thus, the so-called law of the conservation of energy is by its nature incapable of direct empirical validation; our reason for employing it is that it leads logically to empirical laws.

These considerations bring us to a contemplation of a new aspect of physical science. What does it signify to say that the basic postulates of a science need not necessarily be laws which are capable of a direct empirical validation? Certainly it brings into prospect a new freedom of the investigator in seeking what is to him the most satisfactory form of exposition of a science sufficiently advanced to be subjected to a postulational treatment. If he desires to do so, he may go back of what is directly given in experience and create new "causes" to underlie and "explain" the phenomena which he finds. In a certain sense he can impose his own spirit upon the phenomena. The general conception of such a procedure is usually repugnant to the natural scientist, though in practice he not infrequently resorts to this very method. As already indicated, his use of the law of conservation of energy is of precisely this nature. But we can also find examples of a less comprehensive sort. When the fundamental hypotheses of the atomic theory were introduced they had just this character. It is true that there is nowadays some almost direct empirical evidence for the basic hypothesis; but it was not so in the early history of even the modern atomic theory. The requirement of the covariance of the laws of nature under transformations in the four-dimensional space-time world, in the Einstein theory, has much the same character of freedom from direct empirical validation as that which we are now insisting upon as a possibility for the fundamental propositions in a postulational treatment of science.

Among the advantages of a deductive exposition of science based on a system of postulates is an important one to which we must now give some attention. It enables us to localize the doubt in case the suppose laws of phenomena lead to contradiction with empirica fact or merely to conclusions which are mutually contradictory among themselves. If these contradiction exist and if rigorous thinking concerning phenomen is possible, then the initial error must be in the postulates themselves. Thus we have a small body of propositions among which to find the error. We ar

enabled to concentrate our attention at once upon the kernel of the difficulty.

In the presence of such a situation, we cannot fail to raise the question as to whether the laws of our spirit are conformable to the laws of the external world. It is probably a question which we cannot answer with certainty. But if there is not some conformability between the two then we must conclude that natural science is impossible. This is too repugnant to be accepted by us except under compulsion. Moreover the measure of success already attained in the development of science appears to belie the conclusion. The only course left open to us seems to be to assume this conformability, or at least to act upon the hypothesis that it exists. This we have done in the past, perhaps somewhat unconsciously, and this we shall probably continue to do in the future.

If in a postulational treatment of empirical truth we are faced with a situation involving a contradiction, so that we are under the compulsion of changing the conclusions reached, we have no possibility before us except to change the postulates from which we start. But, as we have already seen from the analysis of our miniature mathematical doctrine, we may change the system of postulates without changing the total body of propositions (in postulates and conclusions) of which our body of doctrine consists. We shall need to replace our given system by one which is not equivalent to it; moreover, we shall have to replace it by one having no concrete interpretation (of any sort) in common with the one which we seek to modify so as to avoid contradiction—a fact which will be apparent on a little reflection.

In proceeding to the task of revising or modifying our system of postulates so that it shall realize the known empirical laws and avoid the consequent proposition or propositions which we now know to be false, it may be that we shall have to reconstruct the system entirely. This will not mean that our previous labor

always been able to test our system of postulates for consistency; and we do this by means which depend for their effectiveness upon the fact that the system of postulates contains undefined terms. (We have illustrated the method by the aid of the miniature mathematical science already considered.) But in the physical sciences we have usually thought of the basic terms in the system of postulates (that is, in the system of as the cause of the motion observed. As his experience widens and he passes toward maturity he finds that there are some things which are not under his power nor under that of others whom he sees. It rains, and neither he nor his associates can prevent it; it is clear, and neither he nor his associates can make it rain. What is more natural in this situation than to do what our primitive ancestors did when they assumed that such phenomena were due to the power and the will of beings somewhat like themselves but endowed with much greater strength and perhaps even with omnipotence? This gave an explanation of phenomena which satisfied their primitive requirements. Thus arose mythology as the first explanation of the nature of things.

It is obvious that such a theory does not lend itself readily to a postulational treatment which is effective. The nature of the gods is not sufficiently well known—and suitable hypotheses concerning them do not seem to be possible—to afford the starting-point of logical deduction. To attribute to them omnipotence does not help the matter—at least so far as obtaining control over nature is concerned. As long as human thought remains on this level the conception of a postulational treatment of truth cannot arise.

Perhaps the first phenomena in which important uniformities were observed were those of day and night and the phenomena of the heavens. These are very striking in their character: they are easily observed: they are the most regular of those which are inevitably impressed upon our thought. To astronomy we must look for the earliest development of scientific

ideas properly so called. The remains of prehistoric peoples show that a certain amount of accurate astronomical observation had been made in an early age. Some of this surely passed into the hands of the Greeks before the beginning or at the dawn of the marvelous scientific development which took place among them. To them, of whom Thales of Miletus (580 B. C.) is regarded as the first, we must go for the earliest advance on the mythological view of nature. Acquired facts and the power of a strictly logical deduction first appeared in the Greek mind, and a postulational treatment of truth then became inevitable.

We must guard against the error of looking upon the earlier (the mythological) developments with too much disfavor. There can be no doubt that these explanations—foreign as they are to our ways of thinking—were very helpful in the earliest organization of observation and in breaking it into suitable parts for analysis. "A false theory which can be compared with facts may be more useful at a given stage of development than a true one beyond the comprehension of the time, and incapable of examination by observation or experiment by any means then known." The Newtonian theory would have been useless to our primitive ancestors; but their mythological explanations brought conviction and directed their thought in helpful ways. The ancient atomic theory bore no direct fruit till it was revived in modern times after the possibility had arisen of subjecting it to experimental test; but it now holds a fundamental position in physical science.

Returning to the ancients, let us consider the rise of postulational methods among them. These methods made their first appearance in geometry but not in the empirical geometry which was first developed. The science had its beginnings, so far as the record goes, in the practices of the Egyptian land surveyors. With them it was purely empirical. They were concerned with working rules by which land might be measured or by which it might be divided so that a father could justly apportion his holdings among his sons. The Egyptians knew only that arithmetic which is indispensable for practical computations and that geometry which practical life constantly demanded; and even this they knew only imperfectly; the rules they gave sometimes led to results that are far from accurate.

But when these empirical truths were transplanted into the Greek mind they received a remarkable fructificaton and entered upon a new life which has since then been intimately connected with the most far-reaching developments of exact thought in every direction in which it has gone. The Greeks were not content with practical rules. They sought to go behind them to the more fundamental principles on which they depended. One result of this was to extend and improve the practical rules themselves; but it is not this upon which we wish now to place the emphasis. Their thought gave birth to the science of geometry in the sense of a body of truth in deductive form.

They accomplished this primarily by means of what they called axioms and we today call postulates or assumptions. The significance which they attached to these basic propositions was different from that which we give to them today; but this need not detain us. The momentous thing for human thought was the fact that a form of postulational treatment had arisen and had been brought clearly into the focus of attention. It presaged the death of all mythological explanation and foreshadowed, however faintly, the day when a postulational treatment of empirical truth would become the ideal of every science.

An incidental matter of great importance should have brief mention in passing. As the Greeks conceived geometry it was a veritable science of the space of experience. Nowadays we look upon the matter in a different light. Besides such geometries as they knew we have the so-called non-euclidean geometries. Abstractly taken, these several geometries are inconsistent with one another. They cannot all have been dictated by the space of experience even though this space released those forces of our spirit which gave them existence. The question of how geometry is related to the space of experience therefore has a different meaning to us from that which it had to the ancients. Its consideration has led to a realization of the fact that the geometry of space varies profoundly with a varying choice of the units of measurement. If we had defined the unit of length as the distance through which a body would fall from rest in a second and had proceeded with this unit to explore the regions on the earth, under the earth and above it, we should have had very different conceptions of the nature of space and the relations of material bodies to it from that which we get by means of our customary units. The space so explored would not appear to us to be euclidean, our laws of motion would be different, and even so fundamental a thing as the principle of causality would not be valid (at least without important modifications in its applications). The nature of the space of experience and the course of phenomena in it, as we represent them to ourselves, would have a greatly changing character if we should modify our units of measurement.

Let us return to the course of science in its development. There was considerable progress in astronomical speculation among the Greeks, especially after the impulse given to it by Pythagoras and his followers. The earlier explanations were often too simple in form for the complexity of the facts; and some of the ancient theories underwent a process of complexification which ultimately broke down under its own weight. The "postulates" which served for a time had finally to be adandoned. But observations accumulated and later a great mass of these fell into the hands of Kepler to bring forth through him a great fruit of new laws. Later Newton was able to set the postulate of universal gravitation back of Kepler's laws and astronomy took on a new form and acquired a new dignity. It became the first science of phenomena (properly so called) to become essentially postulational in the form of its exposition. Every one interested in the progress of thought knows what a fundamental advance in celestial mechanics ensued.

But this is not all. The ideal as to the form of truth implicit already in geometry received new attention when it was first realized in a science of phenomena. Celestial mechanics became the norm by which the state of advancement of other sciences was measured: and they received a great impetus from the effort to

realize in them a new ideal of excellence. Rapidly followed the reduction of terrestrial mechanics to the form of an essentially postulational treatment. The basic propositions were (and are) called laws, rather than postulates, but this need not be allowed to obscure their essential nature. This raised still further the hope and the expectation that all science shall be reduced to the ideal form at this time exhibited in geometry and both celestial and terrestrial mechanics.

We cannot pass over this opportunity without emphasizing anew the fact of the non-passivity of the human spirit in the development of mechanics. In the first place it was actuated by an ideal which prompted the direction of progress; important as this was it is perhaps overshadowed by another fact which has received less attention. We have already intimated it in earlier remarks. The laws of mechanics depend in a profound way on the definition of units of measurement. As J. Rueff has shown so clearly (see reference in next section) this choice has been dictated by the demand of the human spirit that the principle of causality should be maintained. Nature has characteristics which make it possible to maintain this law; but the human spirit has been active (and not passive merely) in selecting the units of measurement so that the principle can be maintained. In a certain sense the human spirit has thus imposed itself upon nature; but nature was such in the first place as to render this possible. There appears to be a real solidarity between the human spirit and the external world.

With the expositions of geometry and mechanics in a postulational form the way was open for the like development of all other sciences. We may pass over them rapidly, indicating only the most salient facts. Progress towards a postulational form has perhaps not been continuous throughout the whole body of science, but it has on the whole been rather rapid. The other divisions of physics were the first to realize more or less fully the new ideal. Chemistry has made considerable progress in this direction but not by any means so much as the theory of electricity and magnetism, to name a branch where the success has been remarkable (since the formulation of Maxwell's theory). Recently physics has been in a state of turmoil in which new basic postulates have been sought and found. The older system was not sufficient for the newer facts and a new system is now in process of construction.

When we pass beyond the domain of the physico-chemical sciences we find progress towards a postulational basis of exposition far less advanced. Geology with all its successes lags far behind. Biology has obtained important laws, that of evolution for instance, but it cannot yet be put in deductive form. Psychology is still further removed from the goal and the confusion which characterizes it today does not promise immediate success. Economics is struggling towards the ideal, as we shall see more fully in the next section. Sociology is only far enough along to be getting its empirical bearings; it is far from the state in which a deductive exposition is possible. And ethics and esthetics are hardly ready to hear with patience the suggestion that they be organized as they must be if a successful and adequate postulational treatment shall become possible.

It may be urged that this ideal which arises through the successes of mathematics and mechanics and the physical sciences is not a suitable one for these other disciplines. This possibility we have already considered briefly. It is sufficient to repeat that such an ideal cannot be harmful to these sciences, that it holds out promise of value to them, and that it is probably correct in its insistence that it is the lack of development of them which makes it now impossible for them to realize this beautiful ideal in the form of their exposition.

§3. *A Postulational Treatment of Economics.* In *Des Sciences physiques aux Sciences morales* (Alcan, Paris, 1922), a little book which has been useful to us in certain preceding parts of this chapter, Jacques Rueff proposes an interesting method for reducing economics to a deductive form. This method is developed under the direct guidance of the ideal afforded by the existent postulational treatments of natural science, especially that of rational mechanics. It seems useful to give a brief sketch of his method since it embodies the results of the latest effort to develop science in the direction which we have considered; moreover, such a sketch will serve for us as a helpful illustration of the methods which we have urged.

Rueff does not propose to develop a complete rational theory of economics. He wishes to show only that such a theory is possible and to examine its nature. For this purpose he announces certain principles which can serve as its basis and shows that the first theorems which follow coincide with empirical laws already discovered. In economics one finds already some chapters which present the main characteristics of a rational (that is, deductive) exposition. Rueff proposes a method for founding such a treatment of the whole science.

It is necessary to begin by defining certain symbols, or terms, and setting up certain postulates (axioms, he calls them) which serve to define the use of these terms and to afford the starting-point of the argument; he wishes to be able to develop in a simple and rigorous manner all the empirical laws already known and to lay the foundations for discovering others which have not yet been suspected. In the definition of terms he is guided largely by the analogy of rational mechanics. The use and definition of the term force in the latter directs him towards his primary definition. We perceive that the movement of objects is brought about by the effort which we exert upon them and we are led to suppose in principle that all the movements of material bodies are brought about by something akin to the effort which we put forth; we are brought to the conception of force as the cause of the motion of material bodies. Now the force of which we are conscious is not suitable, in the form in which it appears in our consciousness, to furnish the central term in a rigorou logical development; in particular, it is not sufficiently clearly defined for mathematical purposes. We mus be able to measure it before we can give it a place i our mathematical equations. There is no dispositio to say that we measure a state of our consciousness o even the effort of which we are conscious. We merel introduce a conventional definition suitable to serv the purpose of argument and to lead to known empiric

laws. We say that the force is defined to be the product of mass by acceleration, that is, the product of mass by the rate of change of velocity with respect to time, or again simply the rate of change of momentum.

If we inquire what it is in the exchange of merchandise, let us say in the movement of merchandise, that takes the place of force in mechanics, we shall be led to the conception of one's "need" as the "force" which causes him to seek to acquire the merchandise he wants. In order to use this conception in a mathematical discussion it is necessary to have a definition of its measure. There is no intention to say that we must find a measure of the need as felt psychologically; that may well be impossible. What is sufficient for the immediate purpose is to be able to assign such a measure to need as will enable us to use it effectively in deriving deductively the empirical laws of economics. It is legitimate to give to need a mathematical definition which will serve this purpose without inquiring whether this definition affords the measure of the need as we actually experience it psychologically; just as it is legitimate in mechanics to form a like definition of force. The only question we have to ask is that of the suitability of the definition to furnish the starting-point of our science.

Again the procedure of mechanics suggests to Rueff the direction in which to look for his definition; and he defines one's need n to be the rate of increase of q with respect to the rate of decrease of p where q denotes the quantity of a given merchandise which one has and p denotes the price at which he can purchase it; mathematically, $n = -dq/dp$.

Then his first postulate reads as follows:

I. The need which we have for a certain merchandise decreases when the quantity which we have of it increases.

The need then decreases from a certain value corresponding to zero possession towards the zero of need when possession rises to a certain point marked by satiety; with further increase of possession the need remains at zero.

Before stating the second postulate it is necessary to introduce the notion and the definition of utility. Again we require merely a conventional definition which will serve properly the purposes of a mathematical development. Now the need n is a certain function $f(q)$ of the quantity q possessed. The utility U for a given possession Q is defined to be the integral from 0 to Q of $f(q)$ with respect to q. (The non-mathematical reader will be able to understand the main points without having a precise conception of the meaning of this definition; it will be sufficient if he remembers that the utility depends in a defined way upon one's need.) Then the second postulate may be stated in the following form:

II. Each individual possesses wealth in a finite quantity and seeks by means of exchanges to acquire wealth in different objects in such a way as to bring to a maximum the sum of the respective utilities of the various sorts of things which he comes to possess.

These two terms, need and utility, and these two postulates afford the starting-point of Rueff's deductions. From them he proceeds to derive some of the empirical laws of economics. We need not follow him further in detail. It is sufficient to indicate the character of what he achieves.

In the first place, from this postulational foundation he deduces several of the recognized fundamental empirical laws of economics. Some of these are the following: When an individual possesses two sorts of merchandise A and B in given quantities their total utility to him is a maximum when the ratio of his need of A to that of B is equal to the price of A with respect to B, and conversely; when the price of a certain sort of merchandise increases, the demand for it decreases; purchasers seek to buy at the lowest price possible; the price of a sort of merchandise tends to take a value such that supply and demand become equal.

These will serve as samples of the results deduced from the given postulates. But the latter will not suffice for the whole of economics. Rueff shows briefly how additional postulates may be introduced in order to arrive at the less central results which belong to the divisions of the subject. He has carried the work far enough to make it apparent that the method is capable of extension to the whole science of economics. Thus he has shown that economics is a science in precisely the same sense as that in which geometry or physics is a science and moreover that it is such that its exposition may be put in the form of that of these sciences.

The value of an exposition of economics in such a form cannot be doubted. It is of the same sort as that of other scientific theories which have been so expounded. It gives us a logical and rigorous explanation of observed phenomena. In this way it answers to a need of our spirit, just as the atomic theory does.

It should enable us to discover economic laws which are verifiable empirically. In this way it should be capable of giving an important impetus to the development of economics; for it is usually much easier to test a law once conceived than to come to suspect its existence in the first place. It shows the existence of fundamental functions and points the way to a study of their properties and perhaps even of their forms; this will make it possible to read new meanings from statistical data. It points the direction in which further investigation is most likely to be useful.

But these laws cannot enable us to predict the distant future; they allow us only to foresee what will immediately follow a given state. After a period of revolution or even after a long period of steady progress the state of society may become so modified that new laws must be sought. This is analogous to what happens in physical science when a new kind of phenomenon is discovered, like radioactivity for instance when first observed; it is necessary to lay anew some part of the foundations to make place for the new structure which becomes necessary.

Such a theory does not directly throw new light on the ultimate character of economic phenomena, on the nature of things economic so to speak, any more than a corresponding development in physical science can yield directly a more penetrating insight into nature. It works by indirect means; it gives us a new tool of discovery; it leads us to unsuspected empirical laws; and it is the revealing of these which constitutes its greatest value in aiding toward a deeper understanding of things.

§4. *The Case of Ethics.* Rueff rightly urges that we should go further with the postulational treatment and bring under its power the sciences of ethics and esthetics. The empirical branch of ethics deals with the maxims or rules of right conduct. At a certain epoch and among a given people these rules are fairly well determined and are generally admitted much in the same way as the empirical laws of the physical sciences are generally admitted. It is true that many people often fail to live in accordance with them; but this does not interfere with their existence as rules of the "should be"; and they deal with what should be rather than with what is. Perhaps the first men had no more conception of duty than they had of causality; both of these grew up with their experience and have taken their present form after a long process of evolution. These moral rules must not be taken in isolation and independent of the life of man; they are a product of the racial experience. Their existence is as certain as that of the laws of physical phenomena. They constitute a moral reality as certainly observed and experienced as anything within our knowledge. Perhaps they could be different; if we had developed in a different way we would probably (almost surely) have formulated different moral laws. The evolution of those which we actually have has been a part of our evolution. You shall not kill; you shall not lie; you shall respect the rights of your neighbor; you shall love your parents; you shall be honest, generous, charitable, industrious, temperate, these are parts of our body of empirical moral laws. They might perhaps have been different;

but such as they are they now exist as part of our social order.

With these laws before us the demands of a rational ethics take some such form as the following: Determine suitable general postulates and definitions, however artificial some of these may be in themselves, of such sort that we may proceed from them by rigorous thinking to such propositions as will include all the empirical laws of ethics and nothing in contradiction with them; and then develop these empirical laws in this way. This means that the empirical laws themselves must be reduced to a definite form in order that the comparison may be effected. In the process of this logical deduction it will almost surely happen that some of these laws which have been only imperfectly formulated will be reduced to more exact form; it will then be necessary to ascertain with precision whether this more exact statement of the laws is actually conformable with experience. In addition to this we may expect also, if the case of the physical sciences leads to correct conjectures, that certain laws will emerge which hitherto had not been apprehended or suspected. It will be particularly important to check these with experience. If they are verified a real advancement is realized. If they are not, it will be necessary to modify the postulates. In this way we may set up an interaction between the postulates and empirical laws which will lead to the development of both. This appears to me to promise much for the advancement of ethics; it has certainly been fundamental in the development of physical science.

The experience of the geometers suggests a possibility which has been brought out clearly by Rueff in an incidental remark. Just as we have several geometries each consistent so far as its own propositions are concerned but incompatible with the other geometries, so may we have several sciences of ethics each entirely free of internal contradictions but incompatible with the others. At most, one of these can be applicable to life at a given time and place; at most one of them can be true in this sense. But one can not refute any one of them by a consideration of its internal structure; for there is no contradiction of its parts with one another. The only way in which one of these false doctrines may be overthrown is to bring experience to bear upon it and thus to ascertain whether it can bear the scrutiny of life. Among several ethics, each consistent in itself, we can choose the right one only by the aid of experience.

Not a few treatments of ethics have been largely rational in form, and some of them almost entirely so. Such a one is the theological ethics based on the will of God as the criterion of right. The hypothesis of a valid moral judgment as to right and wrong in the conscience of everyone has been made to serve a similar purpose. Various other systems have also been constructed; but we need not go into them in further detail. "The reading of a book on ethics is for a scientific mind a source of profound astonishment. The systems are innumerable, the conclusions are unique. Whether the ethics expounded is religious or utilitarian, finalistic or pragmatic, it leads always to a well determined set of rules which are, with small variations, the customary ethics of our civilized world."

From the analysis of our illustrative miniature mathematical science we have seen how this might naturally be so. There is such a thing as two distinct systems of postulates being entirely equivalent in the sense that the total body of propositions contained in one set and its consequences is the same as the corresponding total body of propositions from the other set; the two systems differing only in the order in which they derive the propositions and the connections which they exhibit among them. What is found in the case of ethics is something similar to this though it is not precisely the same thing. The systems are not entirely equivalent. They may yield the same laws of conduct so far as the practical life is concerned, but they are different with respect to some of the more or less philosophical propositions contained in them.

The failure of these systems to be precisely equivalent suggests one further remark. They differ most in the things furthest removed from rules of conduct. Perhaps the reason for this lies in their having gone too far from the actual experiences of life itself to find the postulates (or laws) on which to base their deductions. Perhaps suitable postulates may be found in the body of each comprehensive system or lying close to it to serve as the foundation of the essential parts of that system. If the systems are restricted in the way suggested by this remark they may well approach much nearer to logical equivalence. The answer to the question thus raised lies in the future.

The tendency in the past has been to seek postulates which may be directly established, often by some transcendental argument, whereas the suggestion afforded by experience elsewhere with postulate systems directs us rather to look for such postulates as will lead to the empirical facts without so much regard to the immediate plausibility of the postulates themselves. There was a time when physical science proceeded somewhat as ethics has proceeded in the past—with the help of transcendental arguments. The passing of that day in physical science marked the beginning of an epoch in which its progress has been remarkable. Postulates and definitions without transcendental support have been found more useful than those with it. It appears to me that ethics needs to take a step similar to that which released the development of physical science. Such a procedure may deprive its basis of the glory of the categorical imperative; but this is probably an appendage with which it may dispense without loss. Will the change lead to skepticism? I do not believe so. The explanations will be less transcendental but they will be closer to life; the latter is certainly a gain and the former perhaps is also. The experience of other sciences has shown that the method is helpful elsewhere. Ethics awaits this fundamental step in its development.

CHAPTER V

THE STRUCTURE OF EXACT THOUGHT

An interesting and remarkable experiment in the nature of mathematical thinking and the philosophy of certain thought processes was recently performed at Princeton University by a class of graduate students in Projective Geometry under the direction of Professor O. Veblen.[3] It originated in the desire to emphasize the point that our logical processes may be independent of any particular set of mental images associated with the objects of thought and that they may in fact be performed without any knowledge of concrete objects to which the primitive propositions or postulates refer. The instructor proposed to the class that certain members should make up a set of postulates giving properties sufficient to characterize some set of objects chosen by them and that these postulates should be submitted to the remainder of the class, the latter to proceed to the explicit determination of the nature of the set of objects without any further information concerning them than what was contained in the propositions of the set of postulates. The challenge to these latter students was to make logical deductions from the given postulates and to derive theorems concerning the objects referred to in them until they had learned enough about these objects to know what the

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[3] See *American Mathematical Monthly*, Vol. 29 (Oct., 1922), pp. 357-358.

postulate makers had in mind when they framed the postulates.

Of this experiment, Professor Veblen has written as follows: "The exercise was a success in showing how mathematical deductions can be made without knowledge as to what one is reasoning about. It also brought out vividly the problem of the significance of the logical processes as a method of discovery. While there is a sense in which it is true that you cannot get anything out of a set of postulates except what has been put in them, you can at least find out what was put in them. This is what the solver of this problem has to do in a simple case. In the more complicated case of ordinary geometry, the student is apt to think that he understands what is in the axioms, but every time that he witnesses the derivation of a new theorem it turns out that there was something in them that he had not seen before."

The experiment was not recorded in exactly the form in which it was made owing to the fact that the framers of the system of postulates included in their given system one postulate which could be deduced from the remaining ones and was therefore redundant. This redundant proposition was omitted from the set as printed (loc. cit.). The perfected set of postulates may set forth as follows:

Let S be a class the elements of which are denoted by the symbols A, B, C, ... What these elements are is not stated; but relations among them are to be given in the system of postulates. Furthermore, it is necessary to consider certain undefined sub-classes of S (classes consisting of a part only of the elements of S), any one sub-class of which we shall call an m-class. Two m-classes with no element in common are called conjugates. Concerning the elements and m-classes of S the following properties are postulated:[4]

I. If A and B are distinct elements of S, there is at least one m-class containing both A and B.

II. If A and B are distinct elements of S, there is not more than one m-class containing both A and B.

III. For every m-class there is at least one conjugate m-class.

IV. For every m-class there is not more than one conjugate m-class.

V. There exists at least one m-class.

VI. Every m-class contains at least one element of S.

VII. Every m-class contains not more than a finite number of elements.

The problem set to those to whom these postulates were submitted was to develop the miniature mathematical science based on them and in particular to develop a sufficient number of theorems to prove that the set of assumptions is categorical and to give a concrete interpretation of the set S which satisfies them and to prove that the set is independent. Among the definite questions which would be answered by such an analysis are the following: How many elements are there in S? How many m-classes are there in S, and how are the elements of S distributed among these m-classes? How may it be shown that the answers to the foregoing questions would be different if any one of the

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[4] The reader may compare this set of postulates with that given in Chapter III.

given postulates should be omitted and the remaining six should be retained?

It is recommended to the reader that he investigate the processes of proving theorems by means of abstract propositions by deriving for himself some at least of the consequences of the foregoing set of postulates. That they deal with a subject matter which is almost trivial in itself need not disturb him; for the thing in consideration is not the subject matter of the miniature mathematical science in question, but the processes of thought by which one proceeds from proposition to proposition in dealing with undefined terms and relations.

For the convenience of the reader, we shall state here (without proof) the principal theorems in an order in which they may be readily proved. He is advised to work the theory out for himself before reading the theorems as given here. (1) Every m-class contains at least two elements. (2) The class S contains at least four elements. (3) The class S contains at least six m-classes. (4) No m-class contains more than two elements. (5) The elements of S and the m-classes of S are simply isomorphic with the elements A, B, C, D and the sets AB, AC, AD, BC, BD, CD. (6) Each postulate in the set is independent of the other six.

The processes of thinking involved in the proofs of these propositions are exact in the sense that they lead necessarily to the conclusions stated in the theorems in such way that no one can doubt that the conclusions are true, even though he may not know of any concrete representation or interpretation of the system of postulates at the time when he thinks through the proofs of the theorems. It is important to the philosophy of thought and to the psychology of abstract thinking to have a comprehensive analysis made of the processes by which one reasons in dealing with an abstract doctrine of the sort here indicated.

It is important to realize that such processes of thought are not confined to artificial examples such as that given here. These are the same sort as (a part of) those employed in dealing with any abstract mathematical discipline from the point of view of a postulational treatment. This is precisely the way in which the whole theory is developed in the remarkable *Projective Geometry* of Veblen and Young. It was for the purpose of leading to a better understanding of the processes involved in that extensive work that Professor Veblen proposed the experiment of which we have given an account.

Before leaving this experiment, it is desirable to inject one other remark suggested to Professor Veblen by means of these miniature mathematical sciences. It may be attached to the arguments by which one would develop the theory connected with the set of postulates obtained from the foregoing on replacing the seventh one by the following: No m-class contains more than six elements. It is evident that we should have precisely the same theorems as before. The mathematical science based on the postulates could then be developed completely without counting beyond some small number, let us say, 24. The question arises as to what is the structure of the thought involved in developing this miniature science. How much of logic is needed in this process? If one singles out the principles of logic so employed, will they give us so to speak a miniature logic in the same sense as that in which the set of postulates leads us to a miniature mathematical science? What processes of logic would be omitted if one formed such a miniature logic? "What sort of logic results if the omitted processes are replaced by others?....If it should turn out that the logic required for a satisfying theory of this finite system (or any other particular system as, for example, a particular finite projective space) stops short of that required for larger systems, we would be in the presence of a criterion for the classification of logical processes which might help toward deciding the question as to what logical processes are legitimate in dealing with various types of infinite sets."

To go into a discussion of all of these questions would carry us aside from the main purpose of this chapter. But an analysis of the structure of the thought processes employed in developing a miniature mathematical science and of the principles involved in the logic by which the conclusions are reached does belong definitely to our present enterprise. This we shall pursue further when our main problem is more completely formulated.

Before taking our leave of the example, however, let us consider one more question raised by it, namely, the question as to how nearly all the mathematical theory of a postulational treatment is contained in the postulates themselves or the sense in which it is contained in them. This is a difficult question and has given rise to a great deal of discussion. At one extreme it has been urged that a mathematical science so developed is merely an extended tautology. Others have maintained that actual novelties have appeared and do appear regularly in the development of a mathematical doctrine even when it proceeds by a strict dependence on a postulational basis. There is certainly a sense in which the miniature mathematical science in consideration is contained in the postulates; for, from the postulates alone those who did not know to what they referred were able to proceed to a complete characterization of the science as an abstract science. In what sense, if any, did those who worked out the theorems discover or reveal something about the miniature science in consideration?

Let us first describe the novelties of the process on the lowest possible level and let us see that even on this level the developed science is not a tautology. This can be brought out best by means of a comparison with a corresponding process in physical science. Let us suppose that the chemist is investigating the properties of some particular substance, as lead, for instance. Once he has defined lead sufficiently to enable him always to determine whether a given substance is lead or not, it may be said in one sense that the properties utilized in his definition completely characterize the substance and that in our world as it exists now he has given by his definition all the properties of lead. But it is clear from experience that this is only a partial truth. For this same chemist may proceed with the study of lead and discover many properties of it that he had not at first recognized as its properties. When a piece of lead is given to him he is given implicitly all the properties of lead other than those which belong to it in virtue of its relation to other substances. But so far as his understanding of it is concerned, he is far from being in possession of all the knowledge concerning it which he is able to procure.

So it is with a system of postulates; so it was in particular in the case of the experiment which we have been discussing. In some sense what is implied by the set of postulates is intimately bound up in them; for these implications are seen to be necessary. But with respect to the knower's explicit knowledge of the miniature mathematical science based on the postulates the matter is very different. When he first examines the postulates he is quite unable to see directly what is involved in them. It is only after a step-by-step analysis and reasoning that he can come to see what the situation is in its fullness and in precise interpretation. After such an analysis the knower has a much more precise knowledge of the whole system than any which is conveyed explicitly by the set of postulates as they were first stated. From the point of view of the knower's conception of the science the matter after the argument is certainly very different from what it was before. By reasoning new knowledge has been obtained in just as true a sense as that in which new knowledge is attained by the chemist when he investigates experimentally the properties of lead.

But this is not all. The knowledge of which we have spoken so far is knowledge of the abstract mathematical science which is based on the given set of abstract postulates. There is something of the character of concrete interpretation which is still to be examined. The miniature mathematical science which we have had in consideration is perhaps too simple and too brief to enable us to bring out the point by means of it. Let us go over then to the case of an abstract postulational treatment of Euclidean geometry by means of a set of postulates. For definiteness let us take the famous postulates of Hilbert. These will serve well the purpose of the non-mathematical reader since they have been set forth in a form accessible to such a reader and discussed in considerable fullness by Professor C. J. Keyser in his *Mathematical Philosophy*. The treatment given by Professor Keyser will make abundantly clear in detail the significance of the general remarks which are now to be set forth concerning one element in the character of the results obtainable through postulational treatment.

When one has set up a system of postulates in abstract form to underlie any given comprehensive mathematical disclipine one always finds that these postulates have not only the concrete interpretation from which their author started in formulating them, but also many other concrete interpretations. Some of these latter look very little like that which formed the starting-point of the investigation. It is not difficult for one to convince himself that the number of concrete interpretations is infinite. (See a discussion of this in the book just mentioned.) The investigator does not need to carry all these interpretations in mind, nor indeed to know all of them. when he is carrying out the arguments by means of which the theorems in the abstract science are proved. In fact, as we have seen vividly from the experiment of which we have treated, it is not necessary for the investigator to have in mind any concrete representation of the abstract science at the time when he develops the theorems of the science. The doctrine may be developed in detail with only a single concrete interpretation in mind or even with none at all in mind.

Now as the theorems in this abstract science are developed and the science grows to comprehensive proportions experience has shown that the investigator comes after a time to see additional concrete interpretations of it. He finds other sets of objects, besides those at first in mind, for which all the original postulates are verified. He may do this in many different ways; in fact, he has done so in the case of Euclidean geometry, as Professor Keyser has shown. It is his usual experience in dealing with an abstract mathematical doctrine. For every new interpretation he finds, he knows at once without further analysis that all the theorems already obtained are true. His knowledge here is increasing in a way which is far from tautological. It begins to rise toward the level of creative grandeur: for he is finding, or perhaps creating, comprehensive disciplines whose existence was not seen till he recognized them as present to his thought as concrete interpretations of the abstract science which he was developing.

But this does not yet show the full reach of the novelties which emerge in the process of exact thinking on the basis of a system of postulates. If one examines such a system as that of Hilbert, for instance, or indeed any other which pertains to a comprehensive discipline, he will find that there are some which refer to formative processes, processes by which other objects of thought may be constructed from those objects which are given or have already been constructed.[5] From triangles one may thus construct polygons and then generalize the properties of triangles to corresponding properties of polygons. This is a very simple case of the sort of generalization which is thus possible. One has in fact an unending recurrence in the method by which he passes from objects of thought to new objects. Nor is this all. The formative elements in the system of postulates enable us also to invent new relations by means of the undefined relations which appear in the system; and these can also be extended indefinitely by means of a method of recurrence. The new relations and the new objects may be brought into connection with each other so that one may determine relations among the new objects themselves.

By this extension of the field of objects and the field of relations the base of the mathematical science is greatly enlarged and the whole doctrine obtains a new lease of life. Its development proceeds apace. When the theorems concerning these new objects and new relations become comprehensive in their reach and the science begins to have some appearance of completeness so far as these are concerned, the time is ripe for the production or creation of new objects and new relations; and these are formed. This sort of thing has occurred repeatedly in the development of mathematical science. There is no reason to believe that it need ever come to an end in the case of any

comprehensive system of postulates. It appears that such an exact science may look forward with confidence to an unending development. Certainly there is no indication that the utmost reach is being attained anywhere.

The new objects and relations thus brought into existence in the development of an exact science are just as novel and just as significant (though in a very different way) as are the compounds which the chemist produces from the elements or from simpler compounds. The combinations which are considered are often much more than a mere sum of their parts. Something of the creative power of the mind has been put into them in their formation.

We have said that the development of the abstract science leads to the discovery of new interpretations of it. The matter does not rest there. These new interpretations suggest new theorems; for it often happens that a certain relation obviously exists among the concrete objects whereas the corresponding abstract relation may be quite elusive. The concrete interpretation suggests the abstract theorem and the latter is then established by abstract reasoning. This interplay of the concrete and the abstract becomes of great value in the development of the abstract sciences.

So far we have had before us certain examples of exact thought and have analyzed certain general properties of it without having in mind an explicit definition of it. This we have done deliberately. Before proceeding further, however, it is convenient to state explicitly what we mean by exact thought. By exact thought we mean that sort of thought which is involved in the construction of a doctrinal function. To give meaning to our definition, we shall have to make clear what is meant by a doctrinal function. The idea, which has been in existence for some time, came to crystal clearness when the term to denote it was introduced a short while ago by Professor Keyser in his *Mathematical Philosophy*.

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[5] This remarkable property of comprehensive systems of postulates has been treated with great clarity by L. Rougier in his *La Structure des Théories Déductives.*

A doctrinal function is sufficiently different from other forms of thought that one cannot expect to define it in any other way than by pointing to examples of it and saying, Here is what I mean by doctrinal function. But one can hardly have a just conception of the meaning of exact thought without conceiving that notion clearly. Let us refer again to the miniature abstract mathematical science treated at the beginning of this chapter. That science consists of a number of propositions and the demonstration of some of them by means of the others. These propositions, in their abstract form, are the propositions of a doctrinal function. They are a set of propositions all of which are true for any set of objects for which the postulates are true. This doctrinal function consists of a set of postulates and their necessary logical consequences.

Such is the character of every doctrinal function. Another simple example was given in Chapter III. An important and much more comprehensive example is afforded by the Hilbert postulates already mentioned. They are discussed in detail by Keyser from the point of view of the doctrinal function defined by them. Still another example is given in Chapter VI of the present book.

If we should undertake a partial definition of a

doctrinal function, we should say that it is a body of propositions[6] made up of a consistent set of postulates and the consequences which flow from those postulates by such processes as compel assent to the conclusions reached. It is to be understood that the postulates are abstract in the sense that they refer to undefined terms and relations and hence that all the propositions in the doctrine are abstract in the sense that these undefined terms and relations remain undefined throughout the whole doctrine. Probably the reader can come to a clear conception of "doctrinal function" in no other way than by becoming acquainted with some examples of doctrinal functions by means of a careful development of a body of theorems resulting from some given set of postulates.

If he wants to penetrate rather deeply into the meaning and implications of the notion, he should go even further than this. He should carry the matter far enough to set up for himself a rather simple system of postulates formulated with reference to a given concrete set of related objects, developing the abstract theorems of the resulting miniature mathematical science sufficiently far to enable him to find a new concrete interpretation, that is, one different from that by means of which he constructed his system of postulates in the first place. It may be necessary for him to construct several systems before he can find one capable of satisfying multiple interpretation. But he can hardly suppose that he understands the meaning of the notion of doctrinal function until he is able to do this. And surely he cannot well understand the structure of exact thought until he can make for himself a miniature example of a body of exact thought.

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[6] In the next chapter we shall distinguish between "proposition" and "propositional function." It will be seen that the "propositions" here treated are in reality propositional functions.

One will naturally raise the question as to whether the exercise is worth while. That it is intimately related to mathematics, one of the most extensive and long-pursued enterprises of the human spirit, will probably raise in most minds a presumption in favor of its importance. When we see later in this chapter its intimate connection both with philosophy and with physical science, that presumption will probably give way to conviction. But there is a much nearer reason why the exercise should be of interest to one who is concerned with intellectual matters and the nature of our common human spirit. Here is a process of thinking which has been accounted valid, apparently, by every person who has ever examined it. It has existed (in less explicit form) for millenniums. Its character has never been adequately analyzed from the point of view of the psychology of thinking. No one has ever successfully brought out of it the lessons implicit in it concerning the invariant elements in our common humanity. To be sure, the problem is far removed from the market-place. But it is intimately connected with that which most characterizes man as man, namely, the power to think consistently. Surely, this way of thinking is one with which every intelligent person should wish to be acquainted.

Let us see further in what sense we may call that thinking exact by means of which a doctrinal function is obtained. We shall have to consider the matter of

setting up a system of postulates which is consistent in the sense that no consequences of them can ever be contradictory. We require some direct means of seeing that they are consistent. But first of all we shall have to have the system constructed. How shall this be accomplished? It can hardly be done in any significant case by setting up the postulates at random. One must fix upon some set of objects and form [illegible] the system of postulates by reference to these. Even then the labor will require much care, as a little experimenting will show.

If the postulates are constructed in this way by the aid of a set of objects and if one makes sure that every proposition in the set of postulates is true for that set of objects he may be sure that the system is consistent. One can give no formal proof of this conclusion. But, if thinking is possible at all, we must agree that propositions are consistent if they are all true of a given set of existent things. Again, if the postulates have been formed by one person and another is asked to examine them for consistency, he can proceed only in the following way: If the postulates are inconsistent he may perhaps establish that fact by drawing from them contradictory conclusions; but if they are consistent he can prove this only by exhibiting a set of objects and by seeing directly that each proposition is true of the set of objects constructed or otherwise set forth.

Let us suppose now that a thinker is in possession of a consistent set of postulates and let us inquire as to the character of the processes of thought by means of which he may develop them into a more or less complete doctrinal function. By exact thinking we shall here mean that sort of process by which one obtains from the given postulates and from no other hypotheses some or all of the consequences which flow from those postulates by logical necessity. It is not easy to describe the nature of this logical necessity. The term is appropriate in view of the fact that all competent persons who examine an argument of the sort in question are convinced of the validity of the conclusion reached. There is a sort of necessity either in the nature of the connections of the propositions themselves or in a common bias of the human intellect which carries the thinker forward to the conclusion with conviction. In the presence of the argument one cannot refuse to assent to the cogency of the reasoning. Whatever diversities exist in human thought there is agreement as to the logical necessity of the steps in a rigorous argument. Whatever thinking has this quality of universal necessity so far as the human mind is concerned may be called exact thinking. It is intended that the term shall cover all that kind of thinking which leads necessarily from true propositions to true propositions as well as that kind which proceeds from abstract postulates. The two are in reality aspects of one sort of thinking; and this is characterized by a universal agreement as to its validity.

In speaking of the structure of exact thought we do not mean to imply anything spatial with reference to thought. In fact, the category of space does not apply to thought. The analogy with the structure of things in space is very gross and is apt to become misleading. To compare it with the structure of mind is perhaps more helpful; but there is perhaps danger in this also. By

the structure of thought we mean the totality of connections of its parts and the relations which hold these parts together in the unity of the whole thought. One can perhaps best come to a clear conception of the meaning of the term by observing (as he may objectively) the structure of a doctrinal function—as nearly a product of pure thought as anything we have—and examining the way in which its parts hang together and the character of the connections inherent in it.

In analyzing the character and structure of this exact thinking it is important to avoid the influence of pre-formed judgments. The traditional logic does not afford an adequate view of this exact thinking as a whole. Its structure is far more intricate than the traditional logic presupposes. If an analysis could be made today of the processes of exact thought, free from the influence of the traditional logic, that analysis would deal with the syllogism not as the central elementary process in exact reasoning, but rather as one only—and that not the most important—of the methods by which the mind moves securely from propositions to propositions. The construction of new objects and new relations by means of certain formative principles contained in the postulates occupies a place of more vital importance than the syllogism itself. It is that which gives to a doctrinal function its greatest significance. On account of the unbounded method of recurrence involved in this the doctrinal function in comprehensive cases seems to be capable of an unlimited development. A similar recurrence will also frequently be involved in proof by complete induction.

The thought processes involved in exact thinking, and in particular in the construction of doctrinal functions, have never been subjected to an adequate analysis. The problem is certainly a difficult one, especially if the attempt is made to treat the whole of it at once. It is desirable to have some means of breaking the problem into parts so that detailed information concerning the parts may be procured without the embarrassment due to the presence of the entire problem in its great complexity. It is not easy to find a point of view from which to see the subject in parts of such sort that the analysis of one part can be carried even to a tentative stage of completeness without necessarily bringing in other matters not belonging directly to that part. The existence of the miniature mathematical sciences, of which we have already spoken, suggests a feasible point of departure. There is a certain roundedness and completeness in the development of one of these miniature doctrinal functions, small as it is. One may have considerable confidence that there is the same self-sufficiency in the logical processes involved in the construction of such a doctrinal function. If one proceeds to a study of the logical processes involved in one of these he can have a reasonable confidence in the possibility of pursuing the inquiry to a considerable length without having to bring into account other processes than those which are involved in this miniature discipline. The detailed treatment of this problem in several cases seems to me to be highly desirable; it is certainly accessible to us in the present state of knowledge. But we cannot now pursue the subject further.

Let us turn to another aspect of the structure of exact thought as revealed in the development of a doctrinal function. We have already referred to it briefly. We have seen that, when the investigator finds a new interpretation of a given doctrinal function, he knows at once without further analysis that all the theorems in the doctrinal function are true in the case of this interpretation even though the interpretation was not discovered until after the theorems were proved. This is a matter of great significance. It is borne out by all relevant experience. But our confidence in the truth of the fact is not due to the experimental verification of new interpretations. Even before they are formed we know that, if they are formed, all the theorems of the doctrinal function will be true in the new interpretations. What is it in the nature of a doctrinal function which justifies this confidence in the validity of its propositions for every interpretation which verifies the postulates? As we saw at the beginning of the chapter, a doctrinal function may be developed from its postulates by one who initially knows no concrete interpretation. This makes abundantly clear the fact that the argument proceeds without reference to any interpretation whatever. In fact, it must do so if we are to apply it confidently to a new interpretation of its system of postulates. The argument is carried out without any reference to the meaning of the undefined terms and relations other than what is implicit in the system of postulates. The validity of the doctrinal function is one of form and not one of matter. When a doctrine is derived from the doctrinal function by assigning to its undefined terms and relations certain appropriate concrete meanings, then the validity of this doctrine is one of matter as well as one of form. But the validity of the doctrinal function itself is purely one of form. This fundamental fact is duly stressed by Keyser and by Rougier in the books already cited.

Language is necessary for the transmission of exact thought from one person to another; but the transmission requires also the active participation of the recipient, and this latter in a degree perhaps more marked than in most cases, especially if this exact thought is in the nature of a doctrinal function. A doctrinal function does not live in the words which convey it from one to another; it lives only in the active thought of a mind which conceives it. The words convey a suggestion of the idea but no more than a suggestion. Language is not sufficient to translate the psychological reality in the conception and to convey it in its fullness. In the reduction to words the life of the thought disappears. What is said can at best serve as the starting-point from which one may proceed to reconstruct the thought in his own mind; and this thought is alive only when it is conceived by the thinker. Notwithstanding this general imperfection of language there is one perfection to which it must attain if it is properly to convey the conception of a doctrinal function. It is necessary to get away from metaphor. This is difficult, but it is essential. If a word is to carry a concrete meaning it must carry it definitely and specifically. There must be no doubt as to the exact import which every word has. To be sure, there are undefined terms; but these must be recognized as undefined. The statements made concerning them must be free of all metaphor or other ambiguity. The language must have such unity of meaning that one can never make the error of conceiving it at one time in one sense and at another time in another sense. This is the way to error and inexactness and the route must be avoided. The language is secondary to the doctrinal function as primary; but it is nevertheless of essential importance and must be handled with supreme care.

In confining our attention at present to exact thought there is no desire to disparage other sorts of thought than the exact. There is exact thought which is important and there is that which is unimportant. The same distinction is to be made in the case of inexact thought. Indeed, there are many statements which are equally true of both sorts. Much of what we have just said about language is also applicable in the case of inexact thought; but it is perhaps of not so great importance in this case as in that to which we have applied it. To say that thought is inexact is not to say that it is careless. There exist extensive and carefully developed bodies of inexact thought which come close to some of the profounder meanings of life. There are some things so intimate to our nature that we have not yet been able to analyze them to their elements and are therefore not able to put our thought about them into exact form. These things are sometimes quite as essential to our life and happiness as the air in which we live. The logical connection and dependency which are necessary to exact thought may be absent from a body of thought without decreasing its value.

Some thought is so unsystematic and so unmethodical that it can hardly be said to have any structure at all. The thought in literature and music and various other forms of art is often so unsystematic as to be well-nigh structureless. One principal function of art is to "represent at a glance the whole of its object, and thus produce at once a total effect on the mind of the beholder"; but it is not able to do this by means of exact thought. Indeed, the logical connections of the thought involved are often so slight, at least so far as they appear explicitly, that one may almost lose sight of them altogether. The thought may be difficult and careful and valuable and indeed highly important; but the notion of exactness does not apply to it.

In this inexactness there is a certain implication of formlessness in the thought as such. This does not imply any lack of structure in the forms by which the thought is conveyed. It is necessary to distinguish carefully between the thought and the mould in which it is cast. The thought in art is embodied in forms which are sometimes of elaborate construction, as in the case of some poems, or certain sculpture or architecture; but the thought itself can hardly be said to have a structure at all. It is presented as a unity and not as a whole made up of parts. Analysis may indeed reveal parts. But the intention and character of the work are those belonging to a unit and not to a complex. The thought is set forth by means of conventional forms, perhaps, but it does not itself partake of those forms.

At the opposite extreme from this almost structureless thought, and yet having important elements in common with it, as that of permanence, for instance, is the thought in a postulational treatment of a topic in mathematics with such definiteness of structure so intimate to its character that in it the form of the thought has become the matter of central importance. The character of this structure we have already seen somewhat in detail. Ranging from it towards the thought of art we pass through a considerable domain of exact thought with a definite or fairly definite structure, the quality of exactness being approximated in much of that thought which is not strictly of an exact form. This is particularly true of the thought in theoretical physics.

Whenever a body of thought becomes exact it has a tendency to gravitate into the mathematical field. In fact, wherever such thought appears it has the essential quality of a mathematical discipline; and it has been customary to attach it to the domain of mathematics. Perhaps one would best put the matter in this way: Whenever any particular chapter of thought, whether in physical science or elsewhere, has been reduced to an exact form, the mathematician may seize upon it and by the help which it affords him proceed to the construction of a doctrinal function to which this exact thought shall belong. Thus the appearance of exact thought anywhere makes possible an addition to the existent body of mathematics unless indeed it is true that the corresponding doctrinal function already existed in mathematics. Thus the science of mathematics is repeatedly enriched by these accretions from exact thought wherever it arises. How mathematics in turn repays this debt to science will be seen in part as we proceed.

Since the typical form of exact thought is that of a doctrinal function developed from a system of postulates let us inquire as to what suggestions are afforded to other sciences by the structure of doctrinal functions. The two things which first attract our attention are the undefined terms and relations and the unproved propositions which go to make up the system of postulates. To make the discussion concrete let us consider this matter in relation to the systematic development of economics as a well-reasoned theory. At the very outset one is confronted with the fundamental notion of value and one soon comes to see that the notion is not susceptible of a complete objective and quantitative definition. One is compelled to work with the notion by means of certain characteristic properties which are insufficient to define it as a definitely measurable entity. In the presence of this situation what is the suggestion afforded by the structure of a doctrinal function? Precisely this, that a complete definition is unnecessary. The notion of value may appear in the theory attached to an undefined term. What one requires concerning it is not a definition but a number of primitive propositions which imply certain characteristics of the notion of value. These are directly available. In fact, they are afforded at once by certain empirical laws concerning value. The fact that a doctrinal function can be completely developed and expressed while some of the fundamental notions are undefined thus shows the way in which one may proceed.

Nor is this all. The original propositions need not necessarily be proved. What one will do in the first instance, perhaps, will be to employ empirical laws as the primitive propositions. But as the science develops one will find still simpler propositions which underlie the empirical laws and imply them. These may well be

taken as the primitive propositions in the set of postulates underlying the doctrinal function of which the science of economics is one concrete interpretation. In this way the known structure of doctrinal functions may afford the requisite guidance toward a comprehensive theory. The value of effort in this direction has already been made evident by Rueff in his *Des Sciences physiques aux Sciences morales;* his discussion of the matter is very illuminating.

What is insisted upon here in the case of economics is typical of what may be done in the case of many other disciplines. That it is in fact unconsciously done in effect in the development of many disciplines I have shown in the previous chapters, and I have there given reasons why it seems to me desirable to have the process employed more frequently, more consciously and more systematically. All sciences which aspire to be exact in effect should seek also to attain exactness of form from the point of view of the logical connection of parts in the exposition of them.

What reason have we to believe that there is a consonance between the logical connections of exact thought and the relations of phenomena in the natural world of experience? The question is difficult. The mind has ways of its own to get about among its thoughts and to infer a judgment from other judgments. Phenomena have connections of their own, and the relations among these are those which are due to the properties of the substances with respect to which the phenomena occur. What ground have we to believe that these things of nature are subject to the laws of exact thought and to the ideal of rigorous thinking?

We can hardly expect to settle the question by an immediate insight. Even if one should conclude it in this way his decision would be individual and personal. It would still be necessary to find means to satisfy other persons. Can we in any way subject the matter to experimental determination?

We have always gone on the assumption that whatever is necessary in thought is also necessary in nature, at least when the necessity in thought is of the sort indicated by the following case: Let us suppose that certain propositions are true of the concrete things of experience and let us suppose that we also know a certain logical consequence of these propositions, a new proposition which follows from them by the strict necessity of logical deduction. We have always supposed that the new proposition is true as a matter of fact of the concrete things to which the original propositions referred. We cannot maintain that we have always found these consequential propositions true

sonance of phenomena with exact thought in the sense indicated. But we cannot say that we have found a crucial experiment by means of which to put the matter to a decisive test. The conviction remains with us more as an article of scientific faith than as a conclusion which has been directly demonstrated. The question persists, and it cannot be dismissed by the assertion that it is a fictitious question, that we have separated and opposed things which are conjoined in experience. It is admitted that thought proceeds in the presence of phenomena and that the phenomena contemplated are always those which are present to thought. But it remains true that the laws of thought and the connections of phenomena are at least sufficiently separated to give rise to a real question as to their relations to one another.

It is hardly a satisfactory solution of the problem to say that thinking is impossible unless there is this solidarity between thought and nature. In the first that there is still partakes largely of the nature of faith.

Is it possible ever to remove it from this condition? Or is it desirable to do so? We cannot give conclusive answers to these questions. But it may be desirable to point out one way in which the study of postulate systems makes some approach toward an experimental handling of the problem. Our fundamental test for establishing the consistency of a set of postulates is to exhibit certain objects of such sort that every proposition in the set of postulates is true when taken with reference to this set of objects. Now if the postulates are true in this sense and we should yet be able to derive contradictory propositions from them by means of logical processes alone, we should have to conclude that the properties of the set of verifying objects were mutually contradictory from the point of view of thinking. This would lead to the conclusion that there is a lack of consonance between the connections of thought and those of things. But, as a matter of fact, no such thing has happened in our experience. Its absence argues somewhat in favor of the postulated consonance of thought and things. It comes closer than anything else we know to an experimental confirmation of this deep-lying conviction.

There is a certain important element in the structure of exact thought which is intimately connected with its relations to the development of natural science; of this we shall now speak briefly. There are two distinct classes of propositions in a doctrinal function: those which make up the postulates; and those which make up the consequences of the postulates. When the doctrinal function is given, this separation of the propositions into two classes is not unique. There are numerous ways in which the postulates may be chosen so that the totality of propositions in the doctrinal function shall be unchanged. This matter I have analyzed in detail in the earlier chapters. It is now to be remarked that the application of this fact to physical science brings out the possibility of a great freedom of choice in the basic propositions of a given science. It is desirable to have the basic propositions verified experimentally, either by direct or by indirect means. This freedom in the choice of basic postulates may be exercised in such a way as to make it possible to have the fundamental propositions more directly amenable to experimental verification. One may either reform the corresponding doctrinal function with reference to this ideal, or one may leave the doctrinal function unaltered and submit it to verification by means of certain of the equivalent systems of postulates.

How this advantage may be realized in detail was shown in the case of the restricted theory of relativity in my *Theory of Relativity* (Wiley & Sons, first edition, 1913; second edition, 1920). The matter is presented in such a way that the general situation is readily seen in the special case there in consideration; the reader may be referred to that exposition for the detailed facts.

Whatever pertains immediately to the structure of thought has far-reaching connections in many directions, and this is particularly true of the structure of exact thought. We have just treated its connections with physical science; let us turn next to some of its connections with philosophy. These one can best introduce by a quotation from A. E. Taylor's article on Philosophy in volume 32 of the Twelfth Edition of *The Encyclopaedia Britannica* (1922): "For the present, physics seemed likely to occupy the same sort of central position in philosophical speculation which mathematics had held since 1900." Here we have reference to two important facts which we are now to analyze with respect to the significance of the structure of exact thought. One is this: For the first two decades of the present century mathematics held a sort of central place in philosophical speculation. The second is this: As we enter upon the third decade of the century it begins to appear that physics is coming into the central position in philosophical speculation held by mathematics for the preceding two decades. Let us analyze these facts with reference to the problem which we are now treating.

In what way did mathematics enter into the center of philosophical speculation? The answer to this question lies in part in the self-criticism to which mathematics has subjected its processes of thought in the two preceding generations. An analysis of the meaning and significance of its processes and of the structure of the thought which produced it had become imperative. Even though this task was largely philosophical in its nature it seemed to lie so far outside of the current work of philosophers as to remain undone unless the mathematicians set themselves to this work. Accordingly, there arose an increase of philosophical interest on the part of mathematicians and they began to study the foundations of their science in a broader way than any in which they had been conceived before. The new

orientation of interest brought forth unexpected fruit both for mathematics and for philosophy.

Perhaps this remarkable fruitage may be thought of as consisting principally of two parts. One of these had to do with the conception and theory of infinity and the related matters associated with sets of points. Extensive as was the influence of the ideas arising here, they had probably a less comprehensive reach and were of less importance than certain ideas which moved altogether in another direction. Moreover, the theory of infinity as such is relatively unimportant for our present enterprise. Accordingly, we shall turn from this to the other (and more embracing) stream of influence, namely, that connected with the postulational formulation of mathematical disciplines. In the present chapter and in the two preceeding ones we have seen something of the comprehensive character of the influence of these researches. It is these considerations concerning foundations which have most affected the trend of philosophical thought. They have given us a new conception of the nature of the significance which attaches to a mathematical doctrine. They have led us to a clear notion of the doctrinal function, the highest and most pleasing conception connected with the nature of pure thought.

This element in the influence of mathematics upon the character of philosophical speculation has been intimately associated with its structure as exact thought. It has given us a clearer ideal of the nature of pure thinking. It has afforded a norm for self-criticism on the part of any intellectual discipline. It has shown clearly the creative character of exact thinking, that character in virtue of which it can grow and expand apparently without limit, remaining all the while interesting and significant. It has given the human intellect new ground for believing in its power of consistent thinking. This exact thought, by the very nature of its being, exhibits the inadequacy of every mechanistic explanation of the intellectual life of man. The creative element manifested in the construction of a comprehensive doctrinal function cannot be accounted for on any mechanistic basis. The categories of matter and motion are evidently insufficient to deal with this sort of thought. This fact has not been sufficiently analyzed nor have its corollaries been pressed home to the extent and with the vigor which is so much to be desired.

There has been a tendency on the part of this exact thought itself to creep into the domain of philosophy and to live there in a novel environment. We have been hearing something of late of the scientific method in philosophy. Emphasis has been put upon "the substitution of piecemeal, detailed, and verifiable results for large untested generalities recommended only by a certain appeal to imagination" and this "has gradually crept into philosophy through the critical scrutiny of mathematics." When we pass to these interactions between exact thought and philosophy we are on debatable ground. It is uncertain what is the value of the contributions made in this way. A philosophy inspired by such an ideal is certainly far removed from a "concrete" philosophy in the sense of Croce. But when we remain close to the self-analysis which the character of a doctrinal function forces upon every body of thought and when we criticize all thought in the light

of the ideal afforded by a consideration of doctrinal functions we are in the presence of a profound influence upon all the processes of careful thinking.

If now we turn to physics and inquire in what way it is influencing current philosophical speculation we shall see that the answer is not unique. Our conception of nature and the categories in terms of which nature is to be explained are undergoing profound transformations. This is not the place to take up a general discussion of these various influences. We shall seize upon one only—the one which is perhaps the most important, certainly the one which is most clearly connected with our present enterprise. We refer to those influences which are associated with the recent theory of relativity. The most profound effects of this doctrine on philosophy are closely associated with the relativist demand for the expression of the laws of nature in covariant form. One who looks into this matter with care will see that the demand for covariance is intimately associated with a postulational treatment of phenomena and hence with a treatment of them which inclines greatly toward the ideal of exact thought.

It is not too much to say that a large part of the influence of mathematics and physics upon philosophical speculation is due to their character as exact thought or as an approximation toward exact thought.

In one respect there is a marked contrast between human thought on the one hand and matter and energy on the other. The quantity of each of the latter two—or at least the combined quantity of the two taken together—seems to be a constant in our universe. It suffers neither increase nor decrease. But it is not so with the things of thought. Within the physical world, with its constant sum-total of matter and energy, the human mind has created a new world—a microcosm within the macrocosm. This microcosm is continually on the increase. It becomes enriched in quality and enlarged in quantity. It is this which gives its principal meaning to life and its main dignity to thought. This creation of man's is the most significant result of his energy.

It is intimately associated with that which is central to man's nature. His leading characteristic is the definite power he has of accumulating wealth, both material and intellectual and spiritual, and of transmitting it to his fellows and his children. In this respect he differs more profoundly from other living beings than in any other respect. As Count Korzybski has well said, man is a time-binder, a being who is able to gather up his experiences in time and to capitalize his past in the present and for the future. His law of progress seems to be that of the geometrical ratio. As equal intervals of time are added to his experience he seems to increase his wealth of thought in an approximately fixed ratio. If he can double his mental and spiritual wealth in a certain time, then again in the same length of time he can redouble it, and again he can double it once more in the same length of time, and so on. But we cannot now develop the consequences of this law.

It is obvious that this law of the geometrical increase of mental and spiritual wealth has not always been realized fully in the experience of our race. There have been hindrances to prevent its full play. We shall not go into this matter now; but we shall examine briefly the progress of exact thought from the point of view of the validity of this law.

There is probably nothing else in which the creative power of the human spirit is so well manifested as in the development of a comprehensive doctrinal function. The nature of this activity we have already explained. Here the construction of the microcosm takes its most ideal form. From a small beginning the whole wealth of a comprehensive discipline is opened up to view and becomes a permanent possession of the race. Historically it has been true that the initiation of such a doctrinal function has been a rather slow process. At first the additions are by little and little. But once a considerable body has been developed it serves as a sort of fulcrum by which additional truth may be moved into view. The recent rapid development of mathematical science gives support to the law of the geometrical progression as the law of man's growth. This body of doctrine has increased at a rate which itself has an increasing rate so that the total body of it in our day has become larger than anyone two generations ago could have contemplated as possible.

The element of exactness which enters into the structure of this large body of truth gives it a permanence of a sort which cannot be attained in anything else belonging to human experience. Once a comprehensive doctrinal function has been created it endures without loss as a Form of forms into which an infinitude of concrete doctrines may be moulded. It is a lasting part of the growing microcosm. Its structure is not subject to the destroying forces of time. It not a plaything of circumstance. It is capable of during while mountains pass away. No other structu of man's making can be compared to it in beauty permanence.

CHAPTER VI

THE NOTION OF DOCTRINAL FUNCTION

In his *Mathematical Philosophy,* as we have seen, Professor Cassius J. Keyser has brought to crystal clearness one of the most fundamental notions arising in connection with the modern critical scrutiny of the foundation of mathematics, and has given to it a suggestive name so aptly chosen as to be assured of a permanent place in the literature of mathematics. This is the notion of the doctrinal function. It is abstract in character and somewhat difficult to grasp; but it is so important in the analysis of exact thought that no one interested in a precise understanding of the nature of logical expositions of truth can afford to remain ignorant of it. The notion itself has been long in the process of formation and must have been present with greater or less clarity to the minds of many thinkers. It is implicit in every abstract postulational treatment of a body of truth. An important impulse towards its development was already present in the geometry of Euclid. But the more immediate source of the notion is in the modern critical analysis of the postulational foundations of the several branches or divisions of mathematics. Professor Keyser's contribution consists in his having seized upon a notion which was more or less clear in the minds of many people and in his having brought it out explicitly by a clear definition and having given to it a name so aptly chosen as to seem perfectly fitted to the thing so named. Indeed, my judgment accords fully with that expressed by the eminent Italian mathematician, Professor Gino Loria: "It is my opinion that the notion of Doctrinal Function deserves to be henceforth incorporated in every philosophic exposition of the foundations of mathematics."

Fortunately the notion can be set forth without the use of any previously acquired knowledge of technical mathematics, as we have indeed already seen. In this chapter we shall give a further explanation of it which shall be accessible to any reader of maturity who is willing to think in the abstract terms which are necessary in conveying the idea. The postulational definition of abstract groups of finite order furnishes our starting-point. Thus we shall connect the notion with one of the most important branches of mathematics. But we do not assume on the reader's part any knowledge of the theory of groups. We shall explain from the beginning all the terms and ideas which we shall have occasion to use.

The primary notion which we employ is that of a set, or class, of objects. Such a class may consist of a finite or of an infinite number of objects. For the most part we shall be concerned with sets each of which consists of a finite number of objects. We intend that the word object shall be taken in the broadest possible sense. We are then concerned with mathematical objects, that is, objects so clearly conceived or defined as to be capable of the precise analysis required in mathematics. A few of the more important mathematical objects are those of the following sorts: The different kinds of quantities such as positive integers, rational numbers, complex numbers, matrices; the points, lines, curves, surfaces and solids of geometry; rotations, translations, displacements in general, collineations, and, in fact, geometrical transformations in general; the forces, couples, velocities, and so on, of mechanics; the permutations by means of which various changes may be made in the order of a class of objects, these permutations themselves being regarded as new objects. The reader will have no difficulty in naming still other classes of objects. The logical conception of a class of objects will furnish us with our starting-point in defining groups.

Mathematical objects often present themselves to us not singly but in sets or classes. As examples we may mention: the set of all positive integers; all prime numbers; all lines which meet two given lines in space; all possible arrangements of a given number of letters in linear order; all rotations of a plane about a line perpendicular to it; all rotations of a square into itself; all substitutions that leave a certain expression, or a certain configuration unchanged. It will be observed that this list contains sets having an infinite number of objects and other sets having a finite number of objects. We shall be concerned principally with sets of the latter kind.

Now the objects in the sets which present themselves to our thought may often be combined in pairs so as to give other objects; these resultant objects sometimes belong to the set and sometimes they do not. In a particular set there may be just one rule of combination, or there may be more. With the set of numbers of ordinary algebra we have the two fundamental rules of addition and multiplication. The process by which two points determine a line in geometry may be thought of as a rule of combination of these points to produce a new object of a different sort. The combining of two displacements gives a third displacement. A set of objects, with the associated rule or rules of combination is called a system, or, more explicitly, a mathematical system.

Certain systems which occur very frequently in mathematics and in the most important situations are called groups. The objects which appear in a group we shall call elements. A group may have either a finite number or an infinite number of elements. We shall be concerned principally with groups having a finite number of elements. Such a finite group we shall now define.

Let G be a system consisting of a set of elements and one rule of combination for uniting any pair of them in a given order. Let the symbol $\circ$ denote this rule of combination. Then G is called a finite group if the following five conditions are satisfied:

I. If a and b are any elements of the set, whether the same or different, $a \circ b$ is also an element of the set. [A system satisfying this condition alone is often said to have "the group property."]

II. If a, b, c are any elements of the set, then $(a \circ b) \circ c = a \circ (b \circ c)$; that is, the associative law holds for the given rule of combination and the given set of elements.

III. The set contains an element i, called the identical element or the identity, such that for every element a of the set we have the relation $i \circ a = a \circ i = a$.

IV. If a is any element of the set there is also an element a′ in the set such that $a' \circ a = a \circ a' = i$.

V. The number of elements in the set is finite. [In the case of a particular finite group this number is called the order of the group.]

If the last postulate were omitted we should have the general definition of group, whether finite or infinite. We include this postulate here since we propose now to confine our attention to finite groups.

As an example of a finite group we may take the set of elements consisting of the numbers

$$(1) \qquad +1, -1, +\sqrt{-1}, -\sqrt{-1},$$

ordinary multiplication being the law of combination. The reader will have no difficulty in showing that this system affords a group of order 4.

With this definition of a finite group before us we shall be able to set forth the notion of a doctrinal function. Let us consider the body of statements which is made up by taking the five postulates as given and the logical consequences which may be derived from them. The labors of the mathematicians have already led to a very large number of these consequences and to many applications of them. For the present we leave aside all these applications, fixing our attention upon the body of statements consisting of the postulates and their logical consequences. This body of statements constitutes a doctrinal function. We shall undertake to elucidate the meaning of this notion by a further analysis of what is involved in it. We may now add that in general a doctrinal function is any body of statements which is made up solely of a set of consistent postulates about elements or objects not explicitly defined and the logical consequences which flow from them.

In order that a set of postulates shall be useful for defining a doctrinal function it must be known that the set is consistent in the sense that no two logical deductions from the given postulates can be mutually contradictory. The only known way for establishing the consistency of a set of postulates is to exhibit objects which satisfy them. As applied to the present case this means that we must be able to exhibit at least one finite group. That we have already done, having chosen a very simple one for this purpose. Since we know a system of elements for which each of the postulates in the definition is satisfied we are certain that every logical consequence of these postulates will be a statement which is true of the group that we have exhibited. But no existent thing can have two mutually contradictory properties. Hence no two logical consequences of the set of postulates can be mutually contradictory.

Special types or special cases of finite groups ma be obtained by adjoining to the given five postulat one or more additional ones not implied by them b consistent with them. For instance, we might add as a additional postulate the following: The number o elements in the set is the particular number k. First o all there would be the question of the consistency of th postulate with the others; that is to say, the questio to whether groups exist of every finite order k. N as a matter of fact, the theory of permutations or t

theory of the roots of unity furnishes us a ready means of defining a group of order k for every positive integer k. If we let

$$(2) \qquad 1, w_1, w_2, \ldots, w_{k-1}$$

be the k distinct numbers each of which is a kth root of unity and if we take ordinary multiplication to be the law of combination among them, then the system of elements so formed is readily seen to constitute a group of order k. From this it follows that there is an infinitude of distinct groups each of finite order, and in fact that there is at least one of order k for each given positive integer k. [The number of groups of a given order may be very great, especially if the order is a number having many factors.]

Now suppose that one has derived from the five original postulates certain theorems that are logical consequences of them alone. These theorems must afford valid propositions for each of the infinite totality of existent finite groups. Thus the single argument by means of which these general theorems are derived yields results which apply simultaneously in an infinitude of particular instances. This is one of the great values of a doctrinal function. It often has an infinite number of different interpretations; and for each of these the theorems in it afford valid propositions. There is nothing which surpasses the doctrinal function as a means of procuring economy of thought.

In order to give an example of one of these general theorems let us introduce the notion of a subgroup of a given finite group G. It may happen that a subset, or part, of the elements in G can be selected so that this subset taken alone also forms a group H. We say then that H is a subgroup of G. Thus, in the group named a moment ago as made up of the elements (1), there is a subset of elements, namely +1 and —1, which together form a group of order 2. Hence within the given group of order 4 there is contained a subgroup of order 2. The identity alone forms a group of order 1 and this group is a subgroup of every finite group. But it is often left out of account owing to its trivial character.

One of the most fundamental theorems in the theory of finite groups is to the effect that the order of every subgroup is a factor of the order of the group in which it is contained. Moreover, every group whose order is not a prime number contains one or more subgroups besides itself and the identity. In this theorem we have a statement which gives significant information about every finite group. And the proof of the theorem may be given once for all on the basis of the postulates alone and without reference to the individual groups which exhibit instances of its truth.

The so-called commutative or Abelian groups may be selected out of the totality of finite groups by adding to the five basic postulates the following as a sixth one: If a and b are elements of the set then $a \circ b = b \circ a$. With this condition added we have the basis for a new doctrinal function which is subsumed under the original doctrinal function. It is rich in many new theorems over and above those which belong to the original doctrinal function. In this way we may have a doctrinal function within a doctrinal function and this in turn within another and so on to a long sequence. In each

of them will be valid all the statements contained in any doctrinal function under which it is subsumed: with each further restriction of the field over which the doctrinal function is valid—such restriction being effected by the addition of one or more new postulates not violating the condition of consistency—there will emerge new theorems not always valid for the more comprehensive range. The situation thus illustrated by the theory of finite groups is one which arises in connection with all the more comprehensive doctrinal functions.

So far we have done but little more than exhibit an important doctrinal function and show some of its derivatives or variants. We must now analyze the matter more closely so as to arrive at an understanding of the essential nature of a doctrinal function. We begin with the notion of propositional function—so named by Bertrand Russell—this being the ancestor, so to speak, of the name doctrinal function as proposed by Keyser. "A propositional function is any statement containing one or more real variables . . . to which we can at will assign in any order we please one or more values, or meanings, now one and now another." "By a real variable is meant a name or other symbol whose meaning, or value as we say, is undetermined in the statement" in which it occurs and to which we may give any one of a certain set or class of meanings. Thus in the statement, x is a man, we have the real variable x whose value is not assigned: to it we may give any one of various meanings or values such that the statement x is a man becomes significant, that is, either true or false. If we replace x by the name of some particular man the statement then becomes true; if by the name o a mineral, the statement becomes false. The statemen x is a man therefore sets forth a propositional function As an example of a statement involving several variables we may take the following: x denied that y sai that z confessed to being the author of w. Here w have four real variables.

If one examines the set of postulates by means o which we have set forth the definition of a finite group he will see that they involve real variables, in the sens just defined. In the first place we do not say either what the elements are or what the rule of combination among them is. Indeed we do not even specify the number of elements in the set about which we are speaking in the definition. Concerning the elements and their totality we make certain five statements, these to be taken as postulates or assumptions verified by whatever elements may enter into consideration in any particular case. Since the elements are not named or defined, either individually or as a set, it is clear that the assumed statements about them are of the nature of propositional functions. They are statements involving symbols or names to which we may assign any set of values or meanings so that the statements are true when the terms are so defined.

The statements as they occur are neither true n false. They have the forms of propositions but a not propositions (in the strict sense) since prop sitions are statements which are either true or fal When they are applied to a particular finite group t become propositions and are then true. As the st ments stand they give the forms of five propositio

these five propositional functions become five true propositions for each of the infinitude of particular groups whose existence we have made manifest.

The fact that the statements contained in the postulates are propositional functions rather than propositions has important consequences. The theorems which flow from them by logical processes alone involve real variables, namely, those occurring in the postulates themselves. These deduced theorems are therefore propositional functions. The body of statements which makes up the doctrinal function is therefore a body of propositional functions. This body of propositional functions becomes a body of propositions when and only when the variables in them are given specific values. We have seen, for the case in consideration, that this can be done in an infinitude of ways. The total body of propositional functions resulting from the postulates may be taken as a form of doctrines which reduces to a veritable doctrine when the variables are given any set of appropriate meanings. The doctrinal function is therefore one extended form of many doctrines and reduces to one of them on appropriate determination of the undefined terms.

Concerning the nature of a doctrinal function we have the following paragraph from Keyser's book describing it before he had introduced a name for it: "Let us now think of the postulates and theorems as constituting a Whole—a definite Body of logically related propositional functions. Not one of them is true; not one of them is false. What is true is that *the postulates imply the theorems*. But this statement of implication—though it is a proposition and is a true one—is not a part of the Whole; it is not contained in the Body of functions; were we to put it in, it would stand there alone as an intruder, being neither one of the postulates nor one of the theorems .... neither an implier nor an implied; it is a philosophical proposition *about* the Whole but is not a member of it; it is a critical commentary upon it but not upon itself; it is a judgment,—a just and important judgment,—regarding the Body of propositional functions, but is wholly external to it."

And having further explained the notion of doctrinal function Keyser proceeded as follows to the naming of it and to the reasons for this name: "Our Body of logically related propositional functions, since it is a thing having doctrines for its values, must be named a Doctrinal Function. The same name must, of course, apply to the function body consisting of the postulates of any other postulate system together with the theorems logically deducible from them. It can hardly escape your attention that just as a propositional function has true propositions for its values, a doctrinal function has true doctrines for its values; that just as we viewed a propositional function as the matrix of all the propositions (true or false) derivable from it by substitution of admissible constants, so we may view a doctrinal function as the matrix of all the doctrines (true or false) derivable from it in like manner; and that just as a given propositional function and the propositions derivable from it are identical in form, so a given doctrinal function and the doctrines derivable from it are the same in respect of form; they are *isomorphic*, as we say. In marriage with subject-matter, a Doctrinal Function becomes the matrix of an infinite family of doctrines; the children inherit the form of the mother."

As a body of scientific truth develops into a state such that a strictly logical exposition of it becomes possible it tends more and more towards the character of a doctrinal function. As long as it retains a specific subject-matter it continues to be a doctrine. But it sometimes happens that two or more chapters of science with different subject-matters, come to have each a body of truth whose propositions may take the same form as the corresponding propositions for another subject matter. A notable case of this is afforded by those various subjects which rest in significant part upon the theory of the Laplace differential equation. When such a situation as this arises there is an opportunity, there is a need, for the employment of a doctrinal function to set forth the common form of propositional functions which become the propositions of the several divisions of science when the variables are suitably assigned. This unmeditated gravitation of certain chapters of science towards the form of a doctrinal function suggests, as we have seen in a previous chapter, the desirability of a systematic analysis of the further possible uses of doctrinal functions for economizing thought in natural science. They have served a magnificent purpose in mathematics. The fact that certain parts of natural science have gravitated towards them without the investigator's purpose to work in this direction argues their adaptability to natural science. In this way one of the most deep-lying concepts of mathematics promises to be further useful not only in the development of mathematics itself but also in the logical organization of certain of the more advanced and precise parts of physical science. As exact knowledge increases there will doubtless be an extending use of doctrinal functions.

CHAPTER VII

HYPOTHESIS GROWING INTO VERITABLE PRINCIPLE

The conjectural hypotheses employed in science fall into four classes: those which arise in the mind of a single investigator and are discarded by him without publication because they are seen to be unpromising or even out of accord with facts; those which are put forward but never attain to any real currency; those which, like the hypotheses connected with the theory of phlogiston, were valuable for a long time in directing or suggesting investigations but were finally given up in favor of others which seemed to be more useful; those which persist today in the more exalted form of veritable principles of science. With the lapse of time hypotheses are continually dropping from the fourth class back into the third; and it may even be doubted whether there are any in the fourth class which will remain there indefinitely. It is not intended that this separation into classes shall be contemplated as logical and exhaustive. In fact it can at best be only approximately valid. But it serves to separate hypotheses roughly into groups convenient for use in the present discussion.

Before a given hypothesis can be placed in the fourth group it must undergo a prolonged scrutiny and prove itself of value in a considerable range of investigations. The way in which a hypothesis may pass from the state of a mere conjecture to the position of a veritable principle in science is instructive both to those who are concerned with the advancement of science and to those who meditate on its philosophic implications. Accordingly it seems to be worth while to analyze the stages through which a hypothesis passes in coming to a state of security in science. To some aspects of this problem we shall give our attention in the present chapter.

In order to render the treatment as concrete as possible it seems desirable to have in mind at least one extended development of the sort which we seek to understand. Accordingly I have chosen one of the most important and vitally progressive theories of modern science—the atomic theory—and shall begin the chapter with a brief account of the stages in its development. Its history reaches back to ancient times and its varying fortunes are very interesting and instructive. This theory will furnish the background for the present analysis.

§1. *Development of the Atomic Theory.* It was probably a little earlier than 400 B. C. when Democritus began teaching that the world consists of empty space and an infinite number of small invisible particles or atoms. Both the atoms and their original motions he conceived of as eternal and uncaused: each motion now existent has had its origin in a previous motion. On this basis he built a philosophy of the universe in the form of a mechanical system in which there is no room for a providence or for any kind of intelligent cause working toward an end.

But Democritus was not the first of the Greek physical philosophers to form a conception of the ultimate basis of matter. The question had already been investigated in several aspects with a native power of thought that has hardly been surpassed at any time in the history of physics. That this early work did not go far towards an exact account of the structure of matter appears to have been due to the fact that it was based on too small an accumulation of experience.

Before any progress toward an atomic theory could arise it was necessary that the conception of quantity of matter should be formulated with some precision. The first exact notions of this kind appear to have been based on a consideration of number; and number, in an extended sense, continues to underlie all quantitative investigations. It is necessarily by the help of numbers that concrete quantities are practically measured and calculated. But number, as based on the process of counting in accordance with the conception of the ancients, is discontinuous; one passes, as it were, by a leap or a bound from one integer to the next one. But the magnitudes of abstract geometry are essentially continuous, as the ancients well understood. In attempting to apply measurement to geometrical quantities they were led to the conception of incommensurable quantities, quantities having with the unit no common measure however small. This brought them to a clear conception of the infinite divisibility of space. Some students of these early speculations have concluded that the ancients did not think of time as infinitely divisible in the same sense as space is. "In the days of Zeno of Elea [a finite interval of] time was still regarded as made up of a finite number of moments while space was confessed to be divisible without limit. This was the state of opinion when the celebrated arguments against the possibility of motion, of which that of Achilles and the tortoise is a specimen, were propounded by Zeno, and such, apparently, continued to be the state of opinion until Aristotle pointed out that time is divisible without limit, in precisely the same sense that space is."

With this conception of infinitely divisible space and finitely divisible (or atomic) time dominant in philosophic thought the ancients came to a consideration of the nature of matter. Was it infinitely divisible like space or only finitely divisible like time? It was natural that each of the alternatives should have its supporters. The senses give no direct evidence of a granular structure in matter. The water in a vessel does not have the appearance of being merely a collection of a very large number of particles; the material looks continuous and not granular. "This conception of matter, *as infinitely divisible and continuous,* was taught by Anaxagoras more than four centuries before the Christian era." This philosopher believed that bodies apparently homogeneous and continuous are so in reality. This theory is futile for the purpose of explaining the properties of any substance. If we employ it we can admit observed properties only as ultimate facts; we can not go behind them to the ground of their existence. Notwithstanding this, Aristotle adopted and developed the view of Anaxagoras, denying the existence of discontinuous matter. This became the regnant doctrine in ancient times and persisted in this position throughout the Middle Ages. But it has never been fruitful in scientific discovery.

The precise form of the doctrine of Anaxagoras probably can not be recovered now. A two-sentence summary from the Britannica article reads as follows: "All things have existed in a sort of way from the beginning. But originally they existed in infinitesimally small fragments of themselves, endless in number and inextricably combined throughout the universe." There is no lower limit to the magnitude of these small parts. Democritus, who was born probably thirty or forty years later than Anaxagoras, taught the opposing doctrine as we have already said. Perhaps it had also been formulated earlier; but in the hands of Democritus it took such definite shape as to be often ascribed to him. It has since persisted, finding occasional supporters throughout the long period when the doctrine of Anaxagoras and Aristotle was more in favor.

Democritus taught that atoms are eternal and invisible, that they are absolutely small so that their size can not be diminished, and that they are full and incompressible, being without pores and completely filling the space they occupy. They are all alike in quality, the difference being only apparent—due to the effect of different configurations and combinations of atoms on our sense-impressions. These atoms are separated from one another by the void, that is, by empty and infinitely divisible space.

The atomic theory has always been particularly useful to those philosophers who have tried to explain the activity of matter in terms of exact measurements. Epicurus (342-270 B. C.) taught that we must accept our feelings as they arise from direct observation of matter but that we must also believe much for which we have no direct sense-testimony if only this serves to explain phenomena and in no way runs counter to our sensations. In the infinite space which he conceived he therefore assigned a real existence to an unlimited multitude of indestructible, indivisible and absolutely compact atoms in unbroken motion. Lucretius (circa 98-55 B. C) in his poem *De rerum natura* elaborated the atomic theory with some care, maintaining it with eloquent argument. During the middle ages a few independent thinkers from time to time reasserted the doctrine, but it was submerged by the opposing regnant theory. In the time of Boyle and Newton, however, an atomic theory of matter came again more definitely to the attention of scientific thinkers. Newton regarded a gas as a collection of mutually repellent small particles. To Boyle an element was a substance which could not be separated into other substances; it could enter into combinations with others to form compounds possessing new properties but it could not itself be broken up into simpler components.

Of special importance for the atomic theory, as it afterwards turned out, was the fact that Lavoisier made weighing one of the most important tools of research in chemistry. It was central in those investigations of Dalton (1766-1844) which were concerned with the atomic theory. The idea of the atomic structure of matter probably arose in Dalton's mind as a purely physical conception forced upon him by a study of the expansibility and compressibility of the atmosphere and other gases. Having formulated this hypothesis he assumed that the combinations of these minute particles of matter would take place in the simplest possible way. "He thus arrived at the idea that chemical combination takes place between particles of different weights, and this it was which differentiated his theory from the historic speculations of the Greeks."

Lavoisier showed that chemical combinations or decompositions never change the total weight of the substances involved. "From the point of view of the atomic theory, this obviously means that the weight of individual atoms is not changed in the combinations of atoms which occur in chemical processes. In other words, *the weight of an atom is an invariable quantity.*" This is the first property of atoms to be established by experiment. [Today there is reason to replace this conclusion by a more comprehensive one, but it still remains essentially valid to a high degree of approximation.]

It is not necessary to our purpose to go into the further development of the laws of the atomic theory as they took form in the hands of Dalton and his disciples. What is much more significant for the present discussion is the attitude of Dalton and his early followers towards the theory as a whole. It seems that Dalton thought of his conceptions as a mental representation to be employed for the convenience of the investigator; he did not put his theory forward as explanatory of what actually takes place in nature. "He was led to formulate and employ his atomic theory by pondering over the most convenient manner in which certain chemical facts—the facts of definite and multiple pro-

Writing in 1910, J. J. Thomson said: 'Though the number of elements whose atomic weights are multiples or very nearly so of hydrogen is very striking, there are several which are universally admitted to have atomic weights differing largely from whole numbers. We do not know enough about gravity to say whether this is due to the change of weight of the hydrogen atoms when they combine to form other atoms, or whether the primordial form from which all matter is built up is something other than the hydrogen atom. Whatever may be the nature of this primordial form, the tendency of all recent discoveries has been to emphasize the truth of the conception of a common basis of matter of all kinds."

Later we shall see how the difficulties of Prout's hypothesis have more recently been overcome. For the present we wish to observe that it was set forth at a time when there were no means at hand for its proof and when indeed the facts of experiment, as then known, gave in some cases the most direct evidence against its validity. In fact the negative evidence was so strong that the hypothesis was for a long time given up, at least as an active working suggestion for investigation. Nevertheless it has been revived in our time and is generally admitted now to have a real validity. The discovery of further facts, as we shall see, has removed the main difficulty in the way of its acceptance.

We shall not recount further the earlier steps in the development of the atomic theory. It made its way to a sort of speculative acceptance among chemists long before the physicists came to have a similar confidence in it. Put forward at first as a sort of schematic

representation by means of which to keep in mind the facts of observation, it gained an increasing hold upon the thought of chemists and they began more and more to think of it as a theory relating to realities and not merely as a mnemonic or schematic aid to thought. It is not so very long since the physicist had but little faith in the atom of the chemist. But things have changed and the physicist has now penetrated so deeply into a knowledge of these elements of matter that it is no longer presumption in him to undertake to instruct the chemist in the nature and the action of atoms. Physical methods penetrate more deeply into the atom than the older chemical methods. The physicist has gone so far into the interior of the atom that he can say with confidence that the X-rays originate in its inner part and that 'all atoms have qualitatively the same internal structure, that is to say, the same structure in that part of the atom which is interior to the electronic shells of which the exterior parts of the atoms are composed.

It is a long step from a doubt as to the real existence of the atom to a confident statement as to its structure and the parts in it from which different phenomena arise. This step could not have been taken at once. In fact there is a great intervening period of accumulating investigations. The first which we shall mention is attached to the kinetic theory of gases. This was invented primarily for the purpose of explaining the pressure exerted by gases on the walls of the vessels containing them. "Joule in 1857 actually calculated the velocity with which a particle of hydrogen at ordinary atmospheric pressure and temperature must be moving, portions—and certain physical discoveries . . . could be represented" to thought in such a way as to make it possible to conceive known phenomena in convenient logical associations. Several investigators foresaw the far-reaching complicated problems involved in the atomic view and some of them thought that the theory of Dalton was premature. It did not lead to an exact mathematical relation such as that which underlies the Newtonian theory of gravitation but it did lead scientists to make definite measurements and to put exact analysis where before had been only vague reasoning. In this way it contributed from the first an essential element in the rapid development of chemistry. But it did this as a convenient schematic representation of the facts of experiment rather than as an underlying explanation relating to veritable reality. Its value lay in its aid to the investigator in getting around among his facts and not in any supposed deep-seated connection with things.

While the matter still stood in this hypothetical form another bold speculation appeared which has played an important role in the development of the kinetic theory of gases. Different gases show great similiarity in their physical properties; as an example we may mention the fact that all gases increase in volume by one part in 273 when their temperature (in the usual ranges) is increased by one degree centigrade. Such considerations suggested to the Italian Avogadro a hypothesis which he put forward in 1811, namely, that equal volumes of all gases at the same temperature and presure contain equal numbers of molecules. If this conclusion could be established it would afford the first step toward a direct verification of the atomic view and would stand as one of the most important proofs of its validity. But there were many difficulties in the way. It required a new distinction between molecules and atoms; and in earlier times this seemed to complicate matters. Moreover, Avogadro put forward his hypothesis without announcing any new experimental discoveries; this tended to bring about the neglect which his hypothesis suffered for a long time. He had no proof of its validity. In the state of science when the hypothesis was enunciated there were no apparent means for its proof. It seemed too much like a conjecture which could not be brought into intimate relation with facts. It was a statement as of fact which it seemed to be well-nigh impossible to test by observation. Many years afterwards it came into its own; and it has taken a place of great importance in the modern atomic theory of the constitution of matter.

Another remarkable hypothesis was put forward in 1815, this time by the Englishman Prout. The latter suggested that the hydrogen atoms are the fundamental ones and that the atoms of the other elements consist of a smaller or a larger number of atoms of hydrogen. To begin with there was no direct evidence of the truth of this hypothesis. It was suggested by the fact that the atomic weights of the other elements are almost (or quite) precisely integral multiples of that of hydrogen. This would be well accounted for if it were true that they are merely combinations of hydrogen atoms. But there was no direct way then apparent to test the validity of the hypothesis. In fact there were grave difficulties in the way of its acceptance.

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Later we shall see how the difficulties of Prout's hypothesis have more recently been overcome. For the present we wish to observe that it was set forth at a time when there were no means at hand for its proof and when indeed the facts of experiment, as then known, gave in some cases the most direct evidence against its validity. In fact the negative evidence was so strong that the hypothesis was for a long time given up, at least as an active working suggestion for investigation. Nevertheless it has been revived in our time and is generally admitted now to have a real validity. The discovery of further facts, as we shall see, has removed the main difficulty in the way of its acceptance.

We shall not recount further the earlier steps in the development of the atomic theory. It made its way to a sort of speculative acceptance among chemists long before the physicists came to have a similar confidence in it. Put forward at first as a sort of schematic

representation by means of which to keep in mind the facts of observation, it gained an increasing hold upon the thought of chemists and they began more and more to think of it as a theory relating to realities and not merely as a mnemonic or schematic aid to thought. It is not so very long since the physicist had but little faith in the atom of the chemist. But things have changed and the physicist has now penetrated so deeply into a knowledge of these elements of matter that it is no longer presumption in him to undertake to instruct the chemist in the nature and the action of atoms. Physical methods penetrate more deeply into the atom than the older chemical methods. The physicist has gone so far into the interior of the atom that he can say with confidence that the X-rays originate in its inner part and that 'all atoms have qualitatively the same internal structure, that is to say, the same structure in that part of the atom which is interior to the electronic shells of which the exterior parts of the atoms are composed.

It is a long step from a doubt as to the real existence of the atom to a confident statement as to its structure and the parts in it from which different phenomena arise. This step could not have been taken at once. In fact there is a great intervening period of accumulating investigations. The first which we shall mention is attached to the kinetic theory of gases. This was invented primarily for the purpose of explaining the pressure exerted by gases on the walls of the vessels containing them. "Joule in 1857 actually calculated the velocity with which a particle of hydrogen at ordinary atmospheric pressure and temperature must be moving, assuming that this atmospheric pressure is equilibrated by the rectilinear motion and impact of the supposed particles of the gas on each other and the walls of the containing vessel. This meant taking the atomic view of matter in real earnest, not merely symbolically, as chemists had done." "To establish it still further, there were required definite numerical data as to the size of the smallest particles (henceforth sometimes called atoms, sometimes more correctly molecules) and their number, and also clearer views as to the composition of the molecules out of their elements, the chemical atoms."[7] By means of a certain theory of averages and the hypothesis of random movements of the molecules, the kinetic theory of gases has been developed in detail through use of a rather elaborate mathematical machinery; the results obtained have been in such close agreement with observed facts as to give a rather secure ground for believing in the actual existence and in the rapid motion of the small particles of the gas and of their separation one from the other by relatively large open spaces. The law of Avogadro has been employed freely in the theory and strong reasons have emerged for believing in its validity. The kinetic theory of gases has rendered a great support to the atomic theory of matter.

An observation made by Robert Brown in 1827 le in an interesting way to a vivid conception of the actu existence of the molecules of matter. Examining fre plant cells under the microscope he found in them ce tain small particles which were in continual irregu motion the energy of which seemed to remain unabat

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[7] Merz, *History of European Thought in the Ninetee Century,* volume I, pp. 434, 437.

This was at first supposed to be connected in some way with life; strange as the phenomenon might appear to be, it did not excite any very great interest at the time of its discovery. More recently it has been investigated with great care. It turns out to be a non-vital phenomenon common to all finely divided particles free to move in a liquid in which they are suspended. In the best cases these particles are so small as to be near the limit of visibility in the microscope or even to require the use of the ultra-microscope. These inanimate particles are undergoing a vigorous agitation. As one observes an individual particle he will see it start suddenly into motion or turn quickly in a new direction, pursue its new course for an exceedingly short distance and turn again in another direction.

The explanation offered for this striking phenomenon is the following. The small particles which are visible in the microscope are subject to repeated bombardments from the invisible molecules, and the sudden changes in their motions are due to these molecular impacts. Each bombardment changes the motion of the particle, but most of these changes are too small to be seen. But frequently there is an accumulation of the random bombardments on one side of the visible particle and this sets it agoing in a new way with the result seen by the observer. The motions of the molecules are not seen; but results directly attributable to these motions are observed. The Brownian movements "provide visual demonstration of the reality of the heat-motion postulated by the kinetic theory." In the article on Einstein in the New Volumes of the Encyclopaedia Britannica we read: "One of his earliest publications gave the complete theory and formulae of the phenomenon known as Brownian motion, which had puzzled physicists for nearly eighty years. He showed that the heat motion of particles, which is too small to be perceptible when these particles are large, and which can not be observed in molecules since these themselves are too small, must be perceptible when the particles are just large enough to be visible and gave complete equations which enable the masses themselves to be deduced from the motions of these particles." The molecular theory thus has the support of phenomena almost directly deducible from it with a mathematical precision.

The atomic theory of matter, long apparently unverifiable by direct experiment, has received almost direct proof in a number of ways. The physicists now speak confidently of breaking off particles from the atoms of matter. Elaborate theories have grown up in recent years concerning the internal structure of atoms. The heavier atoms are supposed to be made up each of a large number of components. The Bohr theory of atomic structure has been remarkably successful in the explanation of many phenomena due to the fine structure of matter.

Concerning the general situation as regards atomic theories we have the following passage from the article on the constitution of matter in the New Volumes of the Britannica:

"The essential correctness of the kinetic theory of matter, which assumes that the molecules of matter are in vigorous but irregular motion, has been clearly demonstrated by the experiments of Perrin and others on the motion and equilibrium of small spheres of matter in suspension in fluids which show the Brownian movement. At the same time the atomic or discrete nature of electricity, which had been implicitly assumed in many theories, has received complete experimental verification, and the magnitude of this fundamental unit of charge has been measured with precision. The most accurate experiments on this subject have been made by Millikan by measuring the electric field required to support a small, charged droplet of oil or mercury. The charge on the drop was varied by ionizing the gas in its neighborhood. In this way he has been able to show that the charge always varies by integral multiples of a fundamental unit. The charge given to a drop by friction or any other method is always an integral multiple of this unit charge. This fundamental unit is the same both for positive and negative electricity, and is numerically equal to the charge carried by the negative electron, the positive and negative ions produced in a gas by X-rays, and also to the positive charge carried by the hydrogen atom in the electrolysis of water. The magnitude of this unit charge . . . . and thus the mass of the individual atoms of matter are now known with an accuracy of certainly within one per cent and possibly within one-tenth of one per cent."

From the same article we take also the following paragraph of summary concerning the present state of the atomic theory, now become a veritable electronic theory of matter:

"We have seen that the atom is to be regarded as an electrical structure in which a positively charged nucleus is surrounded by a number of electrons. The magnitude of the nuclear charge and the number of the external electrons are known for each of the elements. In considering the distribution of the external electrons round the nucleus, we are at the outset faced by the great difficulty that no possible arrangement can be permanently stable on the basis of the classical dynamics. For example, an electron in motion round the nucleus must on the classical theory radiate energy and fall into the nucleus. To overcome this fundamental difficulty Bohr has introduced a conception based on the quantum theory, in which radiation only occurs in definite quanta. In this way it is possible to postulate the position of the electrons in the simpler atoms and to calculate their frequency of vibration. The theory of Bohr developed by Sommerfeld and others has achieved remarkable success in explaining many of the details of the spectra of hydrogen and helium both in electric and magnetic fields. Owing, however, to the great complexity of the possible modes of motion when three or more electrons are present, it is difficult to calculate the distribution of the electrons and their modes of vibration in the case of the more complex atoms."

From a study of radioactive transformations considerable information has already been gained concerning the character of the nucleus. It contains both positively charged masses and negative electrons. It has been supposed that helium (of atomic weight 4) is one of the secondary units in the complex structure of the nucleus. But it is clear that there is also an element of mass 1 in the nucleus. To this fundamental unit of structure has been given the name proton. The proton is electrically positive. It is supposed that the nuclei

of all elements are made up of protons and electrons. "The mass of the atom measures the number of protons in the nucleus. This is in a sense a return to the famous hypothesis of Prout in which all the atoms are supposed to be built up of hydrogen as the fundamental unit." "The experimental information at present available is too indefinite to hazard more than a guess as to the nature and magnitude of the forces that come into play when nuclei approach very close to one another, as they must do in the structure of the nucleus of a heavy atom."

A permanent transformation in an atom can be effected only by changing the nucleus. Temporary modifications result from changes in the external shell of electrons. Since it is difficult to penetrate to the nucleus, protected as it is by the external shell of electrons, it is relatively rarely that there is a permanent change in the structure of atoms except in the case of those whose nuclei are not stable. So far we know nothing experimentally about the conditions necessary to the formation of complex nuclei. But there is good reason to believe that "this process of aggregation has gone on in the past, and may be still in progress under favorable conditions, if not on this earth at any rate on some of the stars." We seem to be approaching a state of knowledge in which we shall be able to describe with some confidence the processes of evolution of the atomic elements themselves.

From this rapid sketch of the atomic theory it is clear that it has undergone remarkable changes during its history. At first it was put forward as pure hypothesis or speculation unsustained by any fact of observation or experiment, or at least by any which was in any way conclusive. In this state it existed for a long time, submerged to be sure by the more favored regnant hypothesis that matter is indefinitely divisible but still existing in the minds of a few thinkers who could not get away from it. They kept the theory familiar to many persons, both those who favored it and those who were opposed to it. The atom of Democritus is the ancestor of the modern atom, however much the two may differ in many of their characteristics. In the later forms of the atomic theory as well as in the earlier we have believed with Epicurus many things for which we have had no direct sense-testimony because they have served to explain phenomena and have in no way run counter to our sensations. But as long as the theory failed to come to close grip with facts it was inevitable that it should remain a speculation, interesting to be sure but failing to lead to the discovery of new facts.

When we come to those specific hypotheses of the atomic theory which are more precise in the sense of being subject to number and measurement, in short, are quantitative in character, we find at first a reaching forward of thought to a point far ahead of experience. Avogadro and his contemporaries found no way to test the law which he put forward. But later workers have been able to give a fairly conclusive proof of its validity. Prout's hypothesis was at first so far from demonstration that it soon ceased to attract attention only to be revived recently in a modified form which seems to be essentially valid.

In the early stages of the atomic theory, even in its modern form, there was much doubt among physicists and chemists concerning the real existence of atoms. Some maintained that the theory was only mnemonic or schematic in character and that one should not read into it any further meaning. There were some who went further. They urged that the use of such a scheme was misleading and harmful. The mind was imposing something on nature with which nature herself had nothing to do. This (in their opinion) was harmful and could only hinder the progress of science.

But we have passed far since that day. Step by step the atomic theory came to be taken more truly in earnest. It began to suggest experiments, and these turned out to be profitable. It led even to predictions which were verified by later experience. As it developed it afforded means of explaining phenomena that had hitherto seemed to be removed beyond the possibility of explanation. The atom is now taken in real earnest and is analyzed into an elaborate structure. It is the center around which a very large part of physical science is now revolving and it promises to hold this supreme place during our generation.

§2. *Delacre's View of the Right Position of Theory in Science.* From the foregoing account it is clear that the atomic theory, in its preliminary form, was speculative in character and did not come to grips with reality. With respect to specific facts it was for a long time sterile in the sense that it afforded no help toward the increase of knowledge of phenomena. But more recently, from the time of Dalton to the present, the situation has been different. The theory increased in precision and brought to consideration questions which could be answered only by weighing and measuring. When the theory came into touch with number it was vivified and became an effective living force in scientific development. At first it was held largely as a mnemonic or schematic representation of facts without any claim to close correspondence with reality. But it has grown, as we have seen, in the importance and variety of its contacts with phenomena and workers in chemistry and physics have come to think of it as approximately in agreement with objective reality. In this aspect it has had a profound influence on the development of modern physics and chemistry.

In the present discussion we are not concerned with the atomic theory particularly for its own sake; we are thinking of it rather as affording one notable example of the processes by which a hypothesis grows in acceptance and comes to be received as a veritable principle of nature and is assigned a place in the foundations of an important chapter of science. It is generally agreed that this process takes place rather frequently, that it is legitimate and desirable for science to proceed in this way, and that the principles so brought into play are very important and are in fact essential to the growth of science. But this view of the situation is not without its opponents. In a suggestive little book entitled *"Essai de Philosophie chimique"* (Payot, Paris, 1923). Maurice Delacre has opposed with considerable force some of the current conceptions concerning the place of theory in science and the legitimacy of erecting hypotheses into veritable principles. With some of his main contentions we are in disagreement, believing in fact that the present state

and the historical development of the atomic theory alone may be used to show that his position is not always tenable. But his argument deserves consideration; and, even though we do not accept his main conclusions, we are ready to admit that they will serve a useful purpose in deterring from the opposite extreme from that taken by him. In this section we propose to set forth Delacre's view (in part), according to our understanding of it, and to indicate some reasons why we do not share it.

He opens his introduction with a "profession of faith" which runs somewhat to the following tenor. One single thing is true, the simple and brutal fact, the fact to which the prepossessions of our spirit have added nothing, the fact which the prejudices of the schools have not deformed, the fact which systems and theories have not embodied and disfigured. Every principle, every conception *a priori,* every theory, every system however brilliant they may be, however fruitful they may appear, are only illusions. They do not divine nature; as children of our imagination they are void of reality. Free and independent thought always leads us to verities; rigid systems and principles only estrange us from truth. A scientific theory can not be judged sanely while it is still in the ascendenccy. It is only when a theory has taken its place in history, as the phlogiston theory has done for instance, that we can in full independence appreciate its baneful effects. The glory of Lavoisier rests precisely on the fact that he thought otherwise than the chemists of his time. To urge that theories are useful is to miss the teaching of history. The celebrated theory of phlogiston warped the judgments of all experimenters of the eighteenth century. The great advance had to enlist its first recruits from the young men who had not been contaminated by this seducing error. It is important to arm the beginner with a pitiless scepticism of all theories.

This insistence upon the dominance of the simple fact has many implications with which we are in full agreement. But it appears to us to ignore one important consideration, namely, that of the significance of the separate simple facts. They are meaningless to us unless we see them in relation to each other or to other things. To know that John Lackland passed here today is of no significance to me unless I know something of John Lackland or of what he is about. The totality of simple facts is so great that we cannot consider all of them. We have to find some means of getting about among them and of selecting for our attention those which are worthy of that attention. The basis of this selection rests ultimately upon the prepossessions which guide us—the prepossessions either of scientific theory or of philosophic point of view. We cannot approach with a blank and inactive mind even so simple a thing as an experiment in the laboratory. The record made is a selective one and that selection is based partly upon those things which the mind of the observer brings to the experiment. It is impossible to separate one's self from his previous history and thought and be open solely to the suggestions of the current experiment. These things which the mind brings to the experiment are an essential part of the record—in its color and point of view and often in its significant omissions. Theory clearly formed and conscious or theory unconsciously held will intrude into the record of the experiment in spite of all that the investigator can do. The experimenter never records merely the simple fact at least if he records a verifiable and measured result.

This constant intrusion of past experience and thought even into the bare records of the laboratory itself is a perpetual source of danger; and Delacre's analysis, while it appears to us to go too far, is suggestive of dangers to be avoided and is therefore useful, especially to the beginner in scientific research.

An investigation made simply to verify a theory is not an investigation at all. On experimenting with a system as a guide one does nothing but rediscover the same system: he goes round in a circle. Such is the opinion of Delacre. On this ground he insists upon the futility of all theory. He believes that most scientists nowadays live in an atmosphere of blind confidence in the destiny of the atomic theory and that this theory hinders their work.

The example which he has cited appears to us to be particularly fatal for his purposes, especially if one considers the deeper knowledge of the structure of matter which is emerging in connection with the work of such physicists as Bohr.

The attempt to verify conjectured hypothesis or theory has often resulted in important discovery. It must be admitted that too great confidence in a theory may sometimes conceal what is almost patent in a situation under examination. But one can not entirely avoid the occasional obscuration of a fact whatever method of procedure he may adopt. The best one can do is to reduce the danger to a minimum. When a theory predicts a fact which is afterwards verified, this must be considered a gain even by one who insists upon the dominance of simple fact; and this sort of outcome of theory has been rather frequent. The most notable recent example is afforded by the prediction of Einstein that a light ray passing near a large mass such as the sun would be bent out of its straight course by the gravitational field.

But even when theory predicts what turns out to be not verified there may be a great indirect gain from it. Of course the theory in that case must be modified in some respects; but no investigators are any longer unwilling to modify or even to abandon a theory which fails to be in agreement with facts. A certain theory of the propagation of light through the ether (assumed existent) led to the prediction of certain results. The celebrated experiment of Michelson and Morley showed that these expected results were not existent. This excited great interest and led to considerable turmoil in scientific circles. In fact, it gave one of the primal impulses to the development of the restricted theory of relativity and hence to the general theory in the form which is now continually gaining adherents on account of its marvelous success in explaining and predicting facts. A good theory has a way of reaching forward beyond known facts and of bringing to light new ones to be verified after they are suggested by the theory.

Delacre urges that scepticism is certainly a very good guide in research but that scepticism with regard to an experimentally demonstrated truth is an error. That scepticism is often profitable no one can doubt.

Since there is no such thing as a rigidly demonstrated theory, all deductions based on general considerations must be held as tentative and subject to experimental test. If a fact is established experimentally it must be accepted rather than a theory which is out of agreement with the fact; there can be no room for difference of opinion concerning the validity of this inference. But there is room for error in the hypothesis on which it rests. In the case of the more deep-lying "facts" there is no such thing possible as an absolute experimental demonstration of them. What one does in each case is at most this: he shows that, relative to a certain point of view, they are facts. There is a profound relativity in the more deep-lying facts themselves as well as in the theories which we employ to aid us in bringing our conceptions of phenomena into line.

This relativity of fact is a rather elusive thing and is often overlooked by those who disparage the importance of theory. Our point may be rendered concrete by examining the very example which Delacre cites for the nearly opposite purpose. A certain experimenter labored throughout his life to prove that the hypothesis of Prout is false as to fact. He worked long and earnestly and with great acumen. He made many analyses with unexampled patience and conducted them with a keen intelligence. His memoirs impressed everyone as works of great beauty. The "facts" which he found were not in accordance with Prout's hypothesis. Nevertheless, with the greater knowledge which we possess today, physicists are turning again to Prout's hypothesis and are asserting its essential factual validity. The hypothesis was discarded on the basis of fact and is now being revived on the basis of fact.

How may one account for this phenomenon in the development of thought, a phenomenon which is not isolated but is of frequent occurrence? It appears to me that the explanation rests in the following considerations. In recorded scientific observation, or at least in that part of it which relates to the more delicate phenomena, there is no such thing as a "simple fact." When observations are set forth in the barest way possible there is always wrought into the account a certain element of interpretation. The facts are seen from a certain point of view or are presented with a certain bias. There are always some important considerations which the experimenter does not call into question. These color his findings and contribute no mean part to the character of his "facts."

During the time when people were disposed to discard Prout's hypothesis they accepted without question certain untested or at least unestablished conclusions concerning the nature of atoms and molecules. Our conceptions of the latter have in the mean time undergone profound changes and the whole problem of the atomic theory has been broadened by many new conceptions. Under those aspects of it which accord with the most recent investigations Prout's hypothesis appears in a very different light. The "facts" in the case have been modified by the rise of a new theory. They are not dominant over the theory; but the theory is making new facts. It will be admitted that the last statement must seem paradoxical to one who conceives of facts as absolute entities which science must accept; but if one has a keen sense of the truth that facts themselves are relative to the point of view or to the guiding theory or to the methods of measurement he will find nothing strange or impossible in the conclusion that the facts are partly made by the theories. Between theories and facts there is a mutual relation, and each of the two sets of things is modified by the other.

Delacre describes very clearly a process which is constantly taking place with theories that are growing in favor. A formula is set up for the explanation of observed facts—be it mnemonic or schematic or striving to reach the root of reality. The successes of the formula multiply. The reserve with which it is used becomes less marked. Confidence in it increases. There is a growing tendency to consider its relations to reality as essentially intimate and valid. The hypothesis becomes a principle. As such we teach it to the youths. We build on it as the foundation of logical exposition. From it we deduce facts. We believe in it and agree on many things which we can not or do not test by experience. What was an organic doctrine of science becomes a magnificent machine. It helps us relate phenomena, but it remains a machine. We do not control the principle; the principle conducts our thinking.

All this is correct in the main; but it seems to us to omit one essential thing. Among the conclusions drawn from an accepted principle there are some which are subject to experimental examination and these lead us to put to nature questions which perhaps we would not otherwise have raised. When such a question is answered in accordance with the principle the latter is strengthened; otherwise it is modified so as to be brought into accordance with facts. In either case there is a veritable gain in our knowledge of phenomena. As an example of the thing we have in mind we may cite Maxwell's prediction that a ray of light would be found to exert pressure. This particular conclusion had to wait long for its confirmation, but it was finally the occasion of the experimental demonstration of the conjectured fact.

Now, if a fact which is subject to experimental examination is accepted merely from theoretical considerations and no effort is made to test it in the laboratory, it must be agreed that the theory has been used in an unjustifiable way. That this is too often done must be admitted by every one who is conversant with the relevant parts of the history of science. The point made by Delacre, when suitably modified in the way indicated, must be considered an important one. The "facts" in the case are modified by the theory in mind or, what is much more dangerous, by the uncriticized preconceptions; but each new fact suspected must be carried to nature for her verdict. Only in this way can we procure the proper interaction between theory and fact.

§3. *The Creative Character of Scientific Theory.* The attitude described, and in part opposed, in the preceding section is most likely to be taken by one who has been immersed in the details of experiment in the laboratory and has afterwards approached the history of science with the prepossessions which such work tends to engender. The opposite extreme of attitude might well be expected from one who examines the history of science primarily in the light afforded by

deductive expositions of scientific truth. Since mathematics is the deductive science *par excellence* we should therefore expect the most pronounced form of this opposite attitude among thinkers who have approached the philosophy of science with a background of large mathematical training. The facts appear to bear out this expectation.

To many people it has seemed that much of mathematics is largely creative in character. Its basic conception, that of the positive integer, appears to be of this nature. The early thinker did not find number in external phenomena nor have his successors extracted it thence. Collections or groups are found, to be sure; but the counting of a group of objects is something different from the group itself. Contemplation of groups of things has released a certain activity of the mind and has led to the formation of numbers by man for use in counting things. But the numbers themselves were not in the things. No one has ever found the abstract two, or three, or four, running loose in nature. Number is something wrought into things by the mind itself. It has been created by the human intellect. Experience with things has released the power by which this was done but it has not created this power. Number is a contribution of mind for its own use in getting around among things. It has been created by thinking. These propositions are not such as require demonstration by formal reasoning, and there are some metaphysical prepossessions which may interfere with their acceptance by persons of certain tendencies. But they appear to me to be essentially true. They set out a point of view which seems to me necessary in contemplating the nature of mathematics. Many of the conceptions of this science are essentialy creative in their character. They have grown up in connection with the untrammeled activity of the human mind in following out its own inclinations.

This creative character of mathematics is also apparent in the sort of disciplines constructed. There is an essential freedom in the choice of doctrines. Things do not compel them or even the interest which leads to their construction. It is at once admitted that many branches of mathematics have grown up on account of the questions which have been set by experimental science and even by much less formal experience. But there are chapters of the science which have been developed on account of spontaneous interest in them. The mathematician is essentially free in his choice of subject. The number of topics which he may develop appears to be unlimited; and a new one, as a matter of fact, is added from time to time as interest grows and extends, as the spirit of play suggests, or a sort of spontaneous direction of activity prompts.

When the mathematician sees so much of his own science growing under the impetus of a creative activity he suspects that similar forces are at work in other fields, since all sciences are products of the human intellect, brought into existence under various influences upon an activity which is fundamentally of one type among all thinkers. From this position it is not far to the conclusion that the hypotheses of science are not extracted from that part of nature which is external to the human mind but are invented by the mind through a release of its powers due to its interactions with phenomena. This whole matter turns on the question as to whether the "laws of nature" obtained by induction are dictated entirely by nature or whether the mind in some manner imposes its own bias upon these laws. We can hardly escape the latter conclusion when we contemplate the history and present dominance of a general theory like the atomic theory or the rapid inroads of such a one as the theory of relativity and reflect how these were conceived in the mind before there existed any empirical evidence for them. In such cases as these the mind has imposed its own bias upon the laws of nature or it has had an uncanny foresight of them before they appeared in experimental science. The former seems to be the more natural and justifiable conclusion.

There is much to be said in favor of the thesis that natural science should be considered a construct of the mind rather than a paraphrase of nature wrought out by the mind. The processes of invention are seen most clearly in the formation of hypotheses, and particularly when these are based on only a few observations. It is probably impossible to obtain an experimental proof of the hypothesis or principle of the conservation of energy; and yet all modern physical science is based on it. It seems to be a law imposed by the mind for its convenience but without direct experimental support; at best it is contradicted by no experimental fact, or perhaps it is better to say that all known phenomena can be related by the mind so as to be in accordance with this principle.

The facts seem to require us to go further still. Invention is present even in the process of experimentation. The experimenter is not a mere passive recipient. He actively directs the course of events. He invents phenomena which would be non-existent without his guiding influence. He gives attention to what he wills and ignores other things. He will not see all that happens nor will he record all that he sees. He selects before he puts on record for the examination of others. The mind is a real agent in determining the body of facts on which scientific theory rests.

§4. *Kinds of Hypotheses and Consequent Principles.* In considering the relations of hypotheses to the principles which are evolved from them it is desirable to distinguish between two things both of which have been called by the name hypothesis. In his philosophy of the "As If" (*Die Philosophie des Als Ob*) Vaihinger has insisted that many of the so-called hypotheses of science are not hypotheses in the strict sense but are convenient fictions employed as such by the investigator. We have seen that the atomic hypothesis was used in this way as a guide to many researches before there was general agreement, or even anywhere a definite intention to assume, that the fiction referred to actual realities. Chemical combinations took place *as if* there were a union of definite fixed small particles or atoms. Light moves *as if* it were propagated by an ether having certain properties: and this statement stands whether one discards the ether, as there is now a tendency to do, or believes in its actual existence. These fictions (as opposed to hypotheses) have played a leading part in the scientific, philosophic, religious, and even in the artistic life of man (so Vaihinger argues). We must take this conclusion into account in examining the evolution of hypotheses into principles. There is often a stage when a representation is a fiction even though it afterwards grows into a veritable hypothesis. There has been a certain confusion of thought in including these fictions under the name of hypothesis. We shall now find it convenient to distinguish sharply between a scientific fiction, a proposition of the *as if* type, and a hypothesis more strictly so-called.

The passage from one to the other of these two types of proposition may take place in either order. It is more usual to see a scientific fiction becoming a veritable hypothesis; but the contrary process is sometimes actually found, though it must be admitted that there is a strong tendency to discard a proposition altogether if it has stood for some time as a veritable hypothesis and is forced by facts to give up this high position. The phlogistic theory was once held as appertaining to actual reality. Stahl regarded all substances as capable of resolution into the inflammable principle phlogiston and another material element. In his view the violence of combustion increased in proportion to the amount of phlogiston present. In the process of combustion the phlogiston was liberated. "Metals on calcination gave calces from which the metals could be recovered by adding phlogiston, and experiment showed that this could generally be effected by the action of coal or carbon, which was therefore regarded as practically pure phlogiston; the other constitutent being regarded as an acid." This theory possessed great flexibility and formed the basis of the whole account of chemical action. But when the attention of chemists came to be centered upon weight a strange thing occurred. According to the phlogistic theory a metal loses something when it is calcined, namely phlogiston. But careful weighing showed that the process is accompanied by a gain of something else, namely weight —the increased weight (as we now know) being due to the combined oxygen. This made necessary some change in the phlogistic theory; the difficulty was countered by supposing that phlogiston was a principle of levity and that the increase in weight was due to the liberation of this principle of levity.

An explanation of the latter type is not now in favor. In fact, the phlogistic theory encountered so many difficulties that it has disappeared from the field. But it contained one fundamentally correct idea that had not yet found suitable expression in an enduring form. There is something else besides matter; that something we call energy. The violence of combustion is proportional not to the amount of phlogiston but to the amount of energy liberated. "The vague phlogistic theory, which contained a germ of truth, but one which at that time could not be put into definite terms, had helped to gather up many valuable facts and observations; these were collected and restated in a new and precise language." But with all its values the phlogistic theory was finally discarded. It seems to have held the field for some time after its full service had been rendered; and in this latter period of its existence it seems to have hindered progress.

But I have heard more than one eminent chemist say that a very considerable part of the facts of chemistry could still be well stated by the aid of a suitably conceived phlogiston. That chemists are not accustomed to

use it nowadays does not interfere with the fact that many chemical actions take place *as if* they were accompanied by a liberation of a sort of phlogiston. Without doing violence to facts the phlogistic conception might still be employed in a scientific fiction of the *as if* type. But with the progress of knowledge it has seemed more convenient to replace the notion of phlogiston by that of potential energy. The latter is more consonant with our current way of thinking and is therefore more convenient for our purposes. But it is still true that many phenomena occur *as if* a phlogiston were being eliminated from a substance or were being recovered by it.

The various ethers employed speculatively by the physicists have had a curious history. Some who maintained the existence of a plenum as a philosophical principle found in nature's abhorrence of a vacuum a sufficient reason for imagining an all-pervading ether to fill up the parts of space which were not occupied by other material bodies. To Descartes the existence of separated bodies was enough to prove the presence of a continuous medium between them. "But besides these high metaphysical necessities for a medium," said Clerk Maxwell, "there were more mundane uses to be fulfilled by aethers. Aethers were invented for the planets to swim in, to constitute electric atmospheres and magnetic effluvia, to convey sensations from one part of our bodies to another, and so on, till all space had been filled three or four times over with aethers. It is only when we remember the extensive and mischievous influence on science which hypotheses about aethers used formerly to exercise, that we can appreciate the horror of aethers which sober minded men had during the eighteenth century." But "those who imagined aethers in order to explain phenomena could not specify the nature of the motion of these media, and could not prove that the media, as imagined by them, would produce the effects they were meant to explain." Newton tried to account for gravitation by pressures in an ether; but he refrained from publishing this theory "because he was not able from experiment and observation to give a satisfactory account of this medium, and the manner of its operation in producing the chief phenomena of nature."

It is clear that an actual demonstration of the existence of these media could never have been given. One could not say more than that the phenomena took place *as if* they were supported by a medium of certain postulated properties. Of the various ethers invented only one has survived—that one introduced by Huygens to explain the propagation of light. It has been made to serve many purposes, and the explanations of physics have been expounded by the use of it. There was a time not long since when physicists seemed to think of it as actually existing and as being essential to their theories. More lately there has been a strong disposition to deny its existence. For a while it was supposed to refer to a veritable reality, but it is now treated by many more as a fiction of the *as if* type. As to its status in the immediate future there seems now to be much doubt. It can certainly serve a good purpose still as a hypothesis of the *as if* type. Probably that is as far as it can go. In the theory of the ether, then, we seem to be in a state of oscillation between two

conceptions, the conception of it as a fiction and that which treats it as appertaining to a genuine reality.

In the theory of ethers there was an apparent progress in the direction of a veritable principle of nature, a feeling for a time that such a state had been reached, then a period of doubt and lack of confidence in its correspondence to a veritable reality, so that the whole matter now seems to rest in a state of uncertainty. In the case of the atomic theory the situation has been different. There was a long period in which the hypothesis remained merely speculative; but, when it did begin to come to grips with facts, it started on a march of conquest which has not been broken; and it stands to-day at the very heart of physical science. There begins to be a hope that its development may yet lead us to a full penetrating knowledge of the finer structure of matter, that we shall indeed come to know the structure of all atoms and even the laws of motion of their electronic elements. The atomic hypothesis has grown into a veritable principle of nature, and no evidence is yet in sight that it is likely soon to lose its place of central importance.

That facts exercise a certain dominating effect over theory cannot escape the attention of any one. Indeed many persons have gone too far and have supposed that the theory is dictated by the facts. But when one sees a large revolution in theory without a correspondingly great modification in facts he may be led to suspect that the theory is not uniquely determined by the facts. And when one goes further into the analysis of the situation, as he is certain to do in case he has in mind the systematic analysis of systems of postulates latterly made by the mathematicians, he can not fail to see that there is much room for choice in the selection of the general propositions which underlie the expositions of science.

Once the basic propositions have been selected, the mind, sustained by them, is able to reach forward in the direction of facts not previously recognized. Logical analysis of known facts leads us to unknown facts. There is such a solidarity between mind and things that the former can go unaided from some knowledge of the latter to a greater knowledge. Some basis it must have to start from, but it can sometimes go beyond that basis even to facts not suspected before. Maxwell's prediction of the pressure of light affords a case in point. Another will be afforded by Einstein's prediction of the shift in the lines of the spectrum if this is ultimately verified completely—a result which now seems near attainment. It is this reaching forward of thought to a prospective knowledge of phenomena not previously observed that has led some people to characterize scientific theory as being essentially creative in character.

The theory serves to organize knowledge and to give a relative explanation of the facts. But there is no ultimate explanation possible by the methods of science. Even when a hypothesis has come to be accepted as a veritable principle it still retains much of the *as if* nature which has characterized the early stages of so many theories. It is never impossible to conceive that the basic elements of the theory shall be changed for others. Indeed the large general hypotheses are replaced by others from time to time. It is always logically possible, though sometimes inconvenient, to change the fundamental basis on which the exposition of a science is built. Such possibility is made clear by the analysis of systems of postulates. When the foundations are thus subject to flow there can be nothing ultimate in the character of the general structure. It is true that every structure must give rise to the proper relations among phenomena; but these relations may be presented to our thought by any one of many different general structures. From this it follows that the structures themselves can not be ultimate in character.

There are many considerations which limit and circumscribe the influence that natural science can have upon the wider views of the world and life with which philosophy and religion are concerned. Science is by its nature independent of the opinions which one may hold with regard to the reality behind phenomena. Therefore its special results can not throw light directly upon the nature of any reality which may be supposed to be existent, nor can either its hypotheses or the principles to which they lead exercise a decisive influence in concluding between rival views as to the nature of reality. If one adjoins to natural science certain appropriate philosophical principles and propositions, then the two together may generate conclusions of wide reach. But there can never be anything in natural science to put a categorical negative in the way of transcendental conclusions when these are reached along other paths and are sustained by considerations which lie outside the domain of natural science. The truth of this conclusion will be apparent to one who analyzes fully the way in which hypotheses grow into principles and the relative aspect in which these principles must be viewed and the curious relativity of facts themselves to the broad propositions on which science ultimately rests.

The deepest understanding of nature, human and otherwise, can come only by a sort of sympathy by means of which one enters directly into the experience of others or into an appreciation of the phenomena in consideration. The more mechanical or determined or measured aspects of nature may be subsumed under the concepts of science; but a clear appreciation of the inner aspects of reality must apparently come, if at all, only by a direct sympathy through which one enters immediately into an understanding of things. When we have pressed our hypotheses into the deepest principles which they can yield we still have left a deeper penetration which can be attained only by a sympathy which is supported by an inner vision of the depths of reality.

CHAPTER VIII

WHAT IS REASONING?

§1. *Nature and Classes of Inference.* Inference is the combination of premises, in the form of propositions or propositional functions or judgments, so as to lead to or cause conclusions. Deduction is such a combination as compels a conclusion involved in the premises and following from them of necessity. The main purpose of logic is to direct us how to order judgments into premises so as to yield useful conclusions. It deals with how we infer in fact as well as with how we infer perfectly; and we can not understand its processes fully until we consider inferences of all degrees of probability. Aristotle's classification of the modes of inference presents them in three types: from particular to particular (analogical reasoning); from particular to general (inductive reasoning); from general to particular (deductive reasoning). There are today certain particular methods of reasoning not recognized by Aristotle—the method of mathematical induction, for instance—properly to be included in the class of deductive reasoning. It is in the development of these particular methods of reasoning, rather than in the discovery of a new class of inference, that progress in logic is to be sought, and, in particular, progress in the logic of discovery. "The logician must make such a science of inference as will explain the power and the poverty of human knowledge." "Inference is a deeper a homely "Logic of discovery" is the simple method of nagging

thinking process from judgments to judgment, which only occasionally and partially emerges in the linguistic process from propositions to proposition."

In natural science the logic of discovery consists of "an intimate interlinking of induction and deduction." Creative genius counts for much, especially in the formation of the more far-reaching hypotheses. But keen observation, an inquisitive spirit, and an open eye for a new sort of fact have been very useful. Always in a natural science the first step is the collection of data; and precise measurements must be a part of these before the science can become exact. The analysis of facts, their classification into natural groups, and the summarizing of them into tentative empirical laws must precede all formulation of deep-lying theory. In the construction of the latter it is often necessary to try a variety of hypotheses; and none of them is too far from the mark to be useful if it sets some experiment not conceived without its aid. When two or more hypotheses account for the same class of facts it is a matter of great importance to conceive crucial experiments of such sort that the hypotheses predict different results for each of them. This will surely enable one to choose between the hypotheses or to discard all of them; not more than one of them can survive such a crucial experiment. Step by step new hypotheses are conceived when old ones become inadequate or unsatisfactory and crucial experiments choose between them. In this way one may arrive at a general formula or law of nature having a wide range of validity. At every step in the process of constructing and testing hypotheses the creative imagination counts for much and its highest flights surmount the greatest difficulties.

This process of discovery is not continuous; it does not proceed along an unbroken line. Often there is a great leap to a general impression and afterwards a settling down to specific details suggested by the general impression, followed by a full verification of the details. Round and round the thinking goes from one sort of thought to another, from one side of the spiral to another, with a constant rising along the spiral until some great new thought suddenly moves it to another spiral on which it repeats its experience of varied ascent.

Any adequate description of reasoning must take account of this process of discovery. It is not enough to say how we organize in logical form what we have already found out; indeed it is more important to see how it is that we infer to new truth from the old or in some creative way arrive at new truth.

§2. *Creative Character of Mind.* Something of the character of the creative activities of the mind can be seen in one form from the experience of the mathematicians. Consider, for example, the sequence of acts by which the non-euclidean geometries were created—created, in the sense that they came into being in response to human thought, not having been in being, so far as we have evidence, before they were conceived and developed by the human mind. To think the non-euclidean geometries was, in a large measure, to think in a way contrary to the previous thought of the race. Historically, of course, the successive steps of their development were associated with earlier activities of the mind, the first of them having been along the line of the conventional geometry. But in a fundamental way the new geometries differed from the old. In so far as they differed—and this was in fundamental aspects—they represented a departure from the previous experience of thought. The same sort of thing is realized in every division of knowledge when there is a vitally new discovery of fundamental principles, when there is a realization of a conception having especially novel characteristics. And it must be admitted that such discoveries are made, unless one is willing to maintain—a thing perhaps which few will wish to do—that the human mind has made no fundamental progress in knowledge.

This account of the way of discovery in one of its higher aspects implies a conception of the character of mind according to which one supposes that there is something or some one that is thinking and is unwilling to accept the statement "There is thinking" as embodying all that we may say fundamentally concerning the character of the process of discovery. We are not thinking of the mind as "only a system of beliefs, desires and purposes." We cannot agree that there is no existent entity of any sort to have these beliefs, desires, purposes, or to which they may be attached. To some, apparently, mind is merely the collection of these things, or the stream of them as they flow along; everything of mind is merely a Becoming.

This conception of mental activity and its source, current to-day among a certain class of thinkers, or pseudo-thinkers, appears to me to be one of the strangest things in the history of thought. The mind by means of which this stream of consciousness is contemplated, the mind through which alone these mental processes can have their existence, looks out upon the acts which flow from itself and upon the activities which reside in itself or manifest its characteristics, and, looking upon them, denies its own existence and asserts that there is nothing mental except this system of beliefs, desires, purposes. This act of assertion itself must then be a part of the stream—this stream which is all there is to the mental universe. If one undertakes to think this through clearly to see how such things may be, he must inevitably come to a state of confusion in which he is helpless to reduce his thoughts to a rational connection. He must be willing to remain solely on the empirical plane and not to rise to the higher rational plane where his thoughts may rest free from mutual contradictions. To one who is used to exact thought, those who treat everything of mind as merely a Becoming appear to muddle along and not to know how to reason clearly or consistently; and he can hardly avoid raising the question as to how much of this muddling is an outcome, direct or indirect, of their inadequate conception of the nature of mind.

However this may be, it is certain that one's conception of the character of mind will strongly affect whatever theory he may develop as to the nature of the processes of reasoning and discovery, and will leave its imprint upon whatever logic of discovery he may assent to or formulate.

Illuminating remarks concerning the impossibility of predicting the activity of mind have been made by Clerk Maxwell (see pp. 434-444 of *The Life of James Clerk Maxwell,* by Campbell and Garnett, London, 1882). Among other things he says: "It appears then that in our own nature there are more singular points,—where prediction, except from absolutely perfect data, and guided by the omniscience of contingency, becomes impossible,—than there are in any lower organization. But singular points are by their very nature isolated, and form no appreciable fraction of the continuous course of our existence. Hence predictions of human conduct may be made in many cases. First, with respect to those who have no character at all, especially when considered in crowds, after the statistical method. Second, with respect to individuals of confirmed character, with respect to actions of the kind for which their character is confirmed. If, therefore, those cultivators of physical science from whom the intelligent public deduce their conception of the physicist, and whose style is recognized as marking with a scientific stamp the doctrines they promulgate, are led in pursuit of the arcana of science to the study of the singularities and instabilities, rather than the continuities and the stabilities of things, the promotion of natural knowledge may tend to remove that prejudice in favor of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of that of the past."

Concerning the ways of discovery among mathematicians Henri Poincaré has made some illuminating remarks (translated in *Popular Science Monthly*, September, 1906): "It is impossible to study the works of the great mathematicians, or even those of the lesser, without noticing and distinguishing two opposite tendencies, or rather two entirely different kinds of minds.

The one sort are above all preoccupied with logic; to read their works one is tempted to believe that they have advanced only step by step, after the manner of a Vauban who pushes on his trenches against the place besieged, leaving nothing to chance. The other sort are guided by intuition and at the first stroke make quick but sometimes precarious conquests, like bold cavalry men of the advance guard. The method is not imposed by the matter treated. Though one often says of the first that they are *analysts* and calls the others *geometers*, that does not prevent the one sort from remaining analysts even when they work at geometry, while the others are still geometers even when they occupy themselves with pure analysis. It is the very nature of their minds which makes them logicians or intuitionalists, and they cannot lay it aside when they approach a new subject. The two sorts of mind are equally necessary for the progress of science; both the logicians and the intuitionalists have achieved great things that others could not have done. Who would venture to say whether he preferred that Weierstrass had never written or that there had never been a Riemann? Analysis and synthesis both have their legitimate roles."

§3. *The Role of the Subconscious.* Nearly twenty years ago the French Journal *L'Enseignement mathématique* conducted an inquiry into the habits of mind and the methods of work of a considerable number of mathematicians; the results were published in the journal in 1905 and 1908 and also in a separate volume of 126 pages and later in an enlarged second edition. Notwithstanding the individual differences which are always to be expected in such matters, there was one point of such large agreement that it could not escape the attention of any reader. It had to do with the problem of the origin of mathematical invention, a problem of lively interest to psychology and to a logic of discovery. In mathematical invention the human spirit seems to be less actuated by the things of the external world and to move more nearly under the impulse of its own intrinsic nature than in any other of its activities; therefore by a study of the processes of mathematical thought one can hope to throw important light upon fundamental elements in the ways of discovery.

Mathematical exposition or demonstration moves under the general guidance of reason, but it does not consist in a simple juxtaposition of syllogisms; the latter are placed in a certain order so that their force tends to a definite goal, and the putting of them in order is one of the most important elements in the whole process. It is often through a sort of intuition of their proper order that one is enabled to make a mathematical discovery. But some of the more deep-lying processes of getting them into order are in some way hidden from the investigator himself, so that the mathematical truth appears in his mind from a source which is not open to the direct inspection of consciousness. In some way there appears to be a sort of subconscious activity which examines various possibilities only a few of which—the more promising ones—appear in the field of consciousness. Of the source from which they come the investigator often has no immediate awareness.

It may be profitable to examine the exact statements

of several persons concerning their experiences with these hidden activities. These are taken from the book already mentioned. "Ordinarily the solutions come to me when I am occupied with something entirely different." "The solution sought in vain in the study often comes to me while walking or while engaged in some other occupation which does not completely absorb the thought." "A happy thought, born in the mind, I know not how" often yields the solution previously sought in vain. "The truths which I have discovered have appeared to me most often as if born in my mind." "I believe that I have found all my results, not by chance, but by careful reflection in a determined direction. It has not been by act of will that I have obtained my better ideas, but they have come to me after everything had been prepared by reflection and a period of rest had intervened—often of days, sometimes of weeks,... often they have appeared when I was so little cognizant of them that I could not afterwards assign the time of their arrival; often also they have come when I was reading other and very different things." "The rare new propositions which I have been able to establish have most often seemed to result, if not by chance, at least by a sort of unconscious labor of cerebral incubation." "I believe that in the discovery of every result or theorem of value there is operating a certain law or mysterious function, proper to each individual and suitably designated by the name inspiration." "My principal discoveries present themselves to me spontaneously." "Here is a case the authenticity of which I can guarantee: My mother and her sister, who were rivals in geometry at school, had in the evening sought in vain the solution of a problem. During the night my mother discovered the solution of the problem and began to develop it aloud in a clear voice. My aunt, hearing her, arose and made a note of the solution. The next morning at class my aunt was found to have the proper solution, while my mother was still unable to solve the problem."

Such are the experiences of a few people in dealing with mathematical questions. There are others who have nothing of this sort to record. But the testimony given is sufficient to point rather strongly to some sort of subconscious activity in the discovery of truth. If certain mathematicians and certain persons working in other fields do not have such experiences, it nevertheless remains true that there are those who have them, or at least seem to themselves to have them. It may be that the same elements are present in the work of other people and are so obscured as not to be seen; it is possible that mathematics is sufficiently different from other fields of thought that these elements are sometimes present in it and are not to be found in the other fields, but this seems to be an unsatisfactory explanation; and some may even maintain that the mathematicians quoted are deceived about the character of their own activity in this respect, but to rest the matter with this conclusion seems to me to acknowledge defeat in seeking an explanation.

For my own part, I find my experience in accord with what is indicated by the quotations given.

Poincaré, the most eminent of recent mathematicians and a philosopher of profound power, has put on record (*L'Enseignement mathématique* 10 (1908):357-

371) the fact that his own experience seems to him to show clearly the importance of these subconscious activities. It may be well to reproduce here in some fullness his account of the matter as he relates it concretely by reference to a particular sequence of experiences. (The reader unacquainted with the technical mathematical terms employed will find that he does not need to know their meaning; they are not important to the understanding of the psychological processes; in fact, for a non-mathematical reader, they may serve merely as tags to distinguish the different parts of the psychological processes described.)

For fifteen days Poincaré tried to demonstrate that no functions exist of the sort which he afterwards discovered and named fuchsian functions. Every day he worked at the problem for an hour or two. He tried a great number of combinations, but without any useful result. One evening he took black coffee, contrary to his custom; he was unable to fall asleep; ideas surged in his mind in crowds; he perceived them jostling each other until two of them joined, as it were, to form a stable combination. By the next morning he found that he had established the existence of a class of fuchsian functions, those which arise from the hypergeometric series. He had nothing left to do with them but to organize the results, a labor requiring only a few hours.

Then he desired to represent these functions as the quotient of two series; this idea was conscious and he reflected upon the problem; he was guided by the analogy with elliptic functions. He asked what must be the properties of these series if they exist, and without difficulty he came to form those series which he called thetafuchsian.

At this point his labors were interrupted by the necessity of taking part in a geological excursion. The incidents of the journey made it necessary for him to leave off his mathematical investigations. In a certain town he desired to take an omnibus. At the moment when he put his foot on the step of the omnibus, the idea came to him (without any conscious preparatory thought) that the transformations of which he had made use in defining the fuchsian functions are in fact identical with those of non-euclidean geometry. He did not then effect a conscious verification; immediately on being seated in the omnibus, he continued the conversation commenced a moment before; but he had a feeling of entire certainty concerning the named identity of transformations. After returning home, he verified the identity consciously and without difficulty.

Afterwards he proceeded to study certain arithmetic questions, at first without any considerable result and without a suspicion that these had the least connection with the earlier researches. Disgusted with the lack of success of his new labors, he went to spend a few days at the seashore and gave his conscious thought to other things. One day while walking on a cliff, the idea came to him, again with the same character of succinctness and of suddenness and of immediate certitude, that the arithmetic transformations of indefinite ternary quadratic forms are identical with those of non-euclidean geometry.

Having returned home, he reflected upon this result and developed its consequences. The example of the quadratic forms showed him that there were other fuchsian groups besides those which correspond to the hypergeometric series; he then saw that he could apply to them the theory of thetafuchian series and that consequently there exist other fuchsian functions besides those which are derived from hypergeometric series, the only ones which he knew up to that time. Then he naturally proposed to himself the problem of forming all such functions: he laid systematic siege to the walls of difficulty, and one after another he took the outer fortifications; but there remained one, nevertheless, which held out against him, one whose fall would place the whole works under his hand. But all of his efforts at this time served only to make clearer the difficulties. All this latter labor had been entirely conscious.

Thereupon he had to leave home for his military service; and for a time he was occupied with things very different from this mathematical investigation. One day while engaged in his duties he found himself suddenly confronted with the solution of the difficulty which (as already indicated) had arrested his mathematical investigations. He did not then develop the matter in detail; and it was only after the conclusion of his military service that he was enabled to proceed with his mathematical investigation. When he did return to his problem he found that he had all the elements of the solution and that he needed only to assemble and order them. He then prepared his definitive memoir on the subject.

Poincaré assures us that this remarkable example is not an isolated thing in his experience. In his other researches he often found analogous experiences. The inquiry into the ways of work of other mathematicians (already referred to) revealed many similar experiences. To those already thus recorded I may perhaps add a like one of my own.

Often I have had a very clear sense of having found a relatively important idea in my thoughts without any consciousness whatever of the source from which it has come. It appears that there is some kind of thinking of a very effective sort going on below the level of consciousness (or is it above that level?) and that when this thinking has been particularly successful it has some way of getting its results registered in consciousness. I remember distinctly one occasion on which I had labored long in the attempt to clear up a difficulty which lay at the center of an investigation that I wished to carry out. I had worked for some days without result, giving almost my whole energy to the problem in hand. But I was making no progress. My wife passed down the hall as I sat at my desk at work in an adjoining room and I remarked to her that it seemed that the difficulty was too great for present success in overcoming it, and I expressed the purpose almost formed to give up the problem—at least for a time.

About fifteen minutes later I seemed to see the whole situation in a new light; I sensed the direction in which the investigation should proceed; I wrote down in tentative form the central theorem around which the investigation should center; and in a few minutes I had prepared an outline of some eight or ten sections giving the organization of the whole memoir which was to contain the results of the investigation. It required some two weeks to obtain the proof of the central theorem which was thus discovered apparently with suddenness. But no essential modifications in it were needed. The whole memoir (of about forty small quarto pages) was written to the outline prepared in those few minutes after the initial illumination; and this outline required no important change even though the memoir was rather rich in the number of its theorems.

There is a curious sense of certainty about the working out of the details of a memoir which has thus been definitely conceived in some moment of apparent illumination—a certainty which is sometimes almost uncanny in its character. If one were disposed to employ the hypothesis of disembodied spirits to explain phenomena of experience and if he supposed that these had some means of reacting upon the mind of one still in the flesh, he would probably be inclined to attribute some of these flashes of inspiration to the illumination of his spirit by the disembodied spirit of another—perhaps of a mathematician who had been carrying on his researches after the separation of his spirit from the body through which his earlier discoveries were communicated to his fellows. But so long as we remain almost totally ignorant of the character and power of the normal subconscious activity we shall probably have no right to turn to so radical a hypothesis as this. What we need first is a careful investigation of this sub-conscious activity—its nature, its power, the conditions of its successful labor. Here is a field of psychology not yet developed—a central part of the psychology of discovery. So far we have found no means to penetrate its mysteries and reduce them to things understood. The nature and operation of these subconscious processes of veritably creative character we have yet no means of investigating. They beckon us to a conquest not yet made.

Although we do not know the nature of these processes and they are still hidden from consciousness by a covering which we can not effectively penetrate except to a very small extent, yet we can say something of the conditions under which the results of these unconscious processes have been attained and have appeared in consciousness. A first essential seems to be a certain preparation for them by means of well-directed conscious labor. Seldom, if ever, do they bring us solutions of problems which have not already been set in our conscious activity. The consciousness is in some way a directive agent for these unconscious processes. It sets them going. But, even when started, they appear to be unable of themselves to carry the work through to a definitive conclusion. Not only must the unconscious labor be preceded by the conscious activity which operates as a directive agency; it must also be followed by a second conscious activity which gathers up and organizes and completes what is presented to it from the realm of the unconscious. These sudden inspirations appear only after voluntary effort—often that which seemed to be unfruitful until its value was realized indirectly through the unconscious processes which it appears in some way to have engendered. The earlier efforts were not sterile, they were necessary to the work of the subconscious activity.

The second conscious labor is needed not only to organize the theory, to deduce the immediate consequences of the main results, and to expound the whole; it is also needed to verify what is presented from the unconscious mind. The sentiment of absolute certitude which accompanies the conscious apprehension of the new truth from the subconscious may also be present when the latter presents us with something which is false. Experience thus teaches us that this feeling of certitude may be deceptive. The deliverances of the subconscious must be put to the test of a cold logical analysis in the clear light of consciousness.

In the present state of knowledge no explanation of this subconscious activity is possible. One may form hypotheses; but one can do no more. Poincaré supposes that the subconscious activity has a prodigious power of forming combinations of ideas and that most of these combinations, since they are useless, are passed by without ever appearing in consciousness. Some of them however, so excite the sense of beauty—the esthetic appreciation so well known to every true mathematician—that they are capable of so stirring the unconscious activities that they arrest our attention and are registered in consciousness. As a minor confirmation, he cites the fact that even when these illuminations are false in their indications they are such as to flatter our natural instinct of mathematical elegance.

§4. *Authority of Man and of Facts.* One feels a certain repugnance toward the thought of assigning such fundamental parts of the process of discovery to the unseen activity of the subconscious. It puts too much of the process beyond the range of direct observation; and up to the present we have found no effective means to study it indirectly. So far as our conscious activity is concerned or our guidance of these hidden processes, they might as well be subject solely to the control, or rather to the lack of control, of chance; and this conception at first is rather repugnant to our thought. But if we look into the matter a little we shall see that chance already has a large place in certain of our more matter-of-fact scientific doctrines. The role of chance in science has been greatly extended during the past half century; and it is now time, as Emile Borel has pointed out, to ask ourselves if we have not witnessed a veritable scientific revolution without perceiving that it has taken place.

To explain a phenomenon statistically is to regard it as the resultant of a great number of unknown phenomena governed by the laws of chance. If one succeeds in explaining thus the law of universal attraction, for instance, one diminishes the mysterious character of this law, so beautiful in its simplicity but so absurd (one must say) in its classic form according to which the attraction is transmitted instantaneously and without intermediary to the most remote distances. The theory of gravitation affords one of the most important current problems of science.

When the theory of probability has been more fully applied to the exact sciences one can expect a greater introduction of the laws of chance into the sciences of biology, sociology, and psychology. By these transformations of science, as Borel has indicated, the notion of scientific verity itself will undergo profound changes, and the dogmatic affirmation of a law of nature will be

replaced by the declaration of the practical impossibility of a miraculous chance. In this process of deriving the laws of nature the multiplicity of separate facts is replaced by a sort of statistical average which the laws of probability impose. The law does not condense a single class of facts in nature, but gives rather a sort of statistical average for which the probability is very great.

This leads us to raise the question as to the circumstances under which it is to be said that a fact has scientific value. Is it necessary that the fact shall first be perceived as affording an instance of a law of general validity or as being one of a set of facts whose statistical average is obtained by means of the laws of probability? Certainly this is necessary before it can take its place in a body of science in a state of systematic development; but it is by no means essential that it shall be known as related to other things in order that it shall have great value as a provoker of thought and as the starting-point of new discoveries. It is true that organized science does not consist of particular facts, however completely they may be listed or however well they may be classified. Organized science results from the discovery of laws by which we are enabled in thought to relate the facts to one another.

The isolated significant fact sets a perpetual question which calls for answer as long as the fact remains isolated and its significance is realized. There is sometimes a tendency to overlook the significance of a fact which shows little immediate evidence of connection with other known facts, particularly if it maintains its isolation for some time. The fact that a piece of amber, on being rubbed, will attract to itself certain small bodies was known to the ancient Greeks, its discovery being attributed to Thales of Miletus. That the mineral ore lodestone attracts iron is mentioned by Lucretius. These two facts form the starting-point, historically, from which the modern science of electromagnetism has been developed. For several centuries these facts remained isolated and their significance for suggestion of investigation was not realized. It was only after they were brought into relation with a vast and intricate complex of phenomena that their importance could be realized. For generations they remained isolated and unstudied; but the stone which was ignored by the builders is now at the corner of the structure of modern science and industry.

This example brings out the impossibility of foreseeing the connections of the isolated fact. As long as it is isolated it has no place in the body of science; but the very fact that it is isolated pleads eloquently for its systematic analysis and investigation.

The question as to the authority which facts should have in natural science is one which the investigator can not refuse to consider. In the Middle Ages it appears that the authority of the Church was well-nigh supreme in many fields of thought and speculation; and later, at the revival of learning, the authority of the ancients came to a place of preeminence over many things not brought under the clutch of the Church. In modern times, and chiefly under an impulse due to natural science, it has become customary to give little credence to the authority of men or of opinion or of institutions; and in place of an appeal to authority there has come to exist in science a universal appeal to facts of the material world. The great success of this appeal in leading to agreement as to what is the truth among certain large ranges of phenomena has perhaps not had a parallel elsewhere in the history of the development of thought; and among workers in natural science there seems to be entire agreement that this appeal to facts is the only one which is legitimate as a matter of last resort.

But why should the appeal to material fact have such a preeminence over the appeal to the judgment of a man of careful thought? What is there about the former more ultimate than the things about the latter? Is it probably true that certain ranges of questions are best answered (in the last resort) by an appeal to facts of a material nature, and certain others by an appeal to the judgement and character of men? Is it likely that in our revolt from authority we have gone to the opposite extreme and have after all a one-sided conception of the matter as well as our forerunners, only on the other side?

In this appeal of natural science from the authority of man to the authority of facts for ultimate support of every conclusion, is there a latent conception that in the last analysis the things of the universe that really count are the material things, that man is a sort of interloper peculiarly prone to error and held straight to the truth only by a constant appeal to what is more primary than himself, the blind forces of some sort of external world which he is trying to understand and into agreement with which he is seeking to bring himself?

In a valuable article Alfred Korzybski has emphasized the interest of a certain division of human history into three periods. The first he calls the Greek, or metaphysical, or pre-scientific period. In this period the observer was the center of all thought and the observed did not matter. The primary ground of truth was in its immediate appeal to the human mind as satisfying and adequate. The second may be called the classical or semi-scientific period when the observer was almost nothing and the only thing that mattered was the observed; most sciences, Korzybski

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of these personalities but is constantly being modified by them. The fundamental thing for science is neither the observer nor the observed, but a sort of union of the two whose interaction affords the raw material of all scientific thought and theory.

Notwithstanding the preeminence of this interaction, I confess that I do not see any means to avoid the appeal to facts in natural science. The relations of material phenomena are to be discovered, not imposed by man. But when it comes to the representation of

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served; and every process of discovery in natural science or in other branches of human knowledge will acquire its best excellence when it is in accordance with this fundamental principle.

§5. *Mind Accounting for Its Processes of Reasoning.* Such, it appears to me, are some of the more elusive elements in the processes of thought which must find an adequate place in every answer to the question, What is reasoning? It can not be assumed in advance that all processes of discovery are carried forward by means of a logic. There is often something of an essentially creative character in the activity of mind, in that reasoning which leads forward to novel results. It can not be doubted that there is some kind of thinking of a very effective sort going on outside of the domain of consciousness; and that the results of it appear in consciousness from time to time; an understanding of these unconscious processes lies beyond the reach of present knowledge. Neither the authority of man alone nor the authority of fact alone is sufficient for determining the validity of all truth; but every process of discovery will acquire its best excellence when it is in accordance with the fundamental principle that the universe, as known to us, is a joint phenomenon of observer and observed.

In the nature of things it is extremely difficult for us to account for our own processes of reasoning. These we can analyze only by aid of the processes themselves. There is room to doubt whether the mind can possibly be able to seize upon and understand and explain its own highest acts. If there is a hierarchy of powers in the mind then the higher might well comprehend the lower; but then there would be no means left for comprehending the highest. A reasoned explanation of reasoning, to be complete, must also explain this explanation—and we seemed forced to an infinite regression. It may very well happen that the lower processes of reasoning can be accounted for by means of the higher, while the highest can be seized upon and understood only by means of a sort of direct sympathy or intuition. Of the latter we should then not have a reasoned account but only a sort of direct (and partial) understanding. Some of the processes we have already referred to in this chapter seem to lie beyond the domain in which we can expect to account for everything by a reasoned explanation.

It is a grave question whether one by a reasoned analysis can come to see the real nature of reasoning itself. If not, can one by some sort of direct sympathy with his own experience understand the nature of his own reasoning and that of others? Whatever the answer, there is certainly something to be gained by carrying the analysis as far as possible.

An important contribution towards the solution of this fundamental problem has been made by E. Rignano in his stimulating book on *The Psychology of Reasoning* (English edition, London and New York, 1923). Probably every analysis will fall short of a full success on account of the inherent difficulty of the task, on the part of mind, of coming to an understanding of its own nature. To us it seems that the contribution made by Rignano, though important, falls short of success in certain aspects which can be made definitely clear. In fact, it appears to us that he has been successful in dealing with none but the simpler forms of reasoning. We do not expect to succeed in bringing the analysis to a stage of completion, but we believe that certain things can be added which bring the problem a step nearer to solution or at least show something which is yet to be done before the treatment can become comprehensive. In the remainder of this chapter we shall undertake to set forth briefly Rignano's theory of reasoning, to indicate its range, to exhibit some problems with which it must have grave difficulty, and to describe at least one type of reasoning (not mentioned by Rignano) to which his theory clearly seems to be inapplicable.

§6. *Rignano's Theory of the Nature of Reasoning.* We can not do better than give the gist of Rignano's theory in his own words. For this purpose we select several short passages which state it in various ways, all to essentially the same tenor. "Experimental verification and reasoning appear then to be really *one and the same process,* in the sense that the second is only the experimental proof itself, imagined instead of actually performed" (page 78). "These few examples purposely chosen among the most varied it was possible to find, could be extended indefinitely and multiplied at will, but they suffice to give a clear idea of what this mental process that we call 'reasoning' essentially is. It would seem to be nothing else, we repeat again, than a *series of operations or experiments simply thought of,* that is to say, operations or experiments that we imagine performed on one or several objects in which we are particularly interested, and that we do not perform actually because, by a series of similar experiments which have been really accomplished in the past, we

already know their respective results. And the final experimental result, 'observed with the mind's eye', to which a similarly connected series of mental experiments leads, constitutes precisely the 'result of the demonstration,' the 'conclusion of reasoning'" (pages 81-82). "We saw that reasoning in essence consists merely of operations or experiments which we confine ourselves to performing in imagination, since we know already what the result of each of them will be, a result which is now *observed,* so to speak, only mentally" (page 141). "None the less, it is clear, as is demonstrated even by the short account of the origin and development of algebraic calculation which we have just given, that these ancient experiments or operations actually performed upon matter still continue to be the one and only substratum of all calculation" (page 153). "It is to be noted before everything else, that the mathematical operation of 'passing to the limit' in no way destroys the fundamental nature of reasoning as a series of imagined operations" (page 170). "Thus mathematics can be defined by saying that it is the science in which the merely imagined experiments, which constitute reasoning, are of a very general quantitative nature, capable of making the most varied physical phenomena equivalent in regard to the quantitative relations thus discovered" (page 194). "Any reasoning whatsoever, as a connected series of imagined experiments, implies in itself for each experiment a corresponding process of induction, a more or less unnoticed induction perhaps, by means of which the result obtained by a given experiment or by several like experiments actually performed in the past, is generalized so that the result is attributed to the present merely imagined experiment, which itself is similar to those which preceded it" (page 196). "None the less these are pure transformations of calculation, which, so far as they satisfy the rules, do not cease—unconsciously for the calculator who employs them henceforth mechanically—always to represent very general experiments of inclusions, adjunctions, intersections, etc., of classes" (page 201). "With this we have completed this rapid, but at the same time perhaps too lengthy excursion into the field of the higher forms of reasoning. It appeared to us necessary, in order to bring clearly into evidence that the fundamental nature of reasoning, as a series of merely imagined operations or experiments, remains without change even in cases where an abstractness pushed to its extremest limit and an excessively complex symbolic form might at first sight succeed in concealing or even disguising this fact" (page 207).

In order to make clearer what Rignano means when he speaks of reasoning as only an experimental proof which is merely imagined instead of being actually performed, let us examine some of the examples which he gives to enforce his point.

He tells us how he was a little surprised when he first heard the statement that it is certain that there are people in London who have just the same number of hairs on their heads. And he gives the reasoning by which he was able to assure himself of the truth of the statement. It is certain that the number of people in London is larger than the greatest total number of hairs a man can have. Starting from this fact Rignano proceeded as follows with his reasoning. He mentally placed the inhabitants of London beside one another, starting with the one having the least number of hairs and putting on his left the one with the next smallest number and so on. He had a vivid conception of seeing all these people lined up before him in single file like so many soldiers for review. Behind the row in the background there appeared a confused mass of the "doubles", those that he had left outside the file because he had found that each of them had the same number of hairs as one of those in the file. He gives us further details concerning the vivid images which he formed in his imagined experiment and concludes the account as follows: "The rapidity with which all these mental operations developed was such that I could say that I had convinced myself of the fact, the bare affirmation of which had at first surprised me, by intuition rather than by reasoning. My intuition was then in this case a very rapid reasoning, almost instantaneous; and this reasoning was nothing else than a series of experiments carried out in the mind alone" (page 73).

A second example of reasoning as an imagined experiment is described by Rignano as follows. He had observed with his own eyes the two following facts: A metal rod transferred from cold to heat becomes longer; a long pendulum swings more slowly than a short one. With these facts in mind, he asks what would happen if he transferred a wall clock having a simple pendulum from a very cold to a very hot room. By reasoning he is able to conclude that the clock, on being transferred to the hot room, will go more slowly. His reasoning, he points out, consists in this: He imagines that the clock is transferred from the cold to the hot room. Thinking of this calls up the vision of an elongated pendulum. But he has seen on another occasion that an elongated pendulum swings more slowly. Since the pendulum regulates the speed of the clock he knows that the clock will go more slowly. He adds: "Thus in this case again my reasoning has only been in substance a series of observations or experiments that I could have performed actually, but that I confined myself to doing in imagination only, because I already knew the result of each in advance" (page 75).

We shall mention still one other of Rignano's examples. Let us consider the reasoning employed in demonstrating that the sum of the angles of a triangle is two right angles. We could measure the three angles and add the results and thus obtain a direct experimental verification. Or, better still, we could form a paper triangle, cut off the three angles, place them side by side so that their vertices coincide, and observe directly that their sum is a straight angle. "Now the ordinary demonstration of the theorem in question (says Rignano) *is simply a mental execution of the same series of experiments.*" He proceeds to analyze the proof in view of this judgment and concludes that the reasoning consists merely of imagined experiments.

The central conception in Rignano's theory must now be apparent to the reader. It is one of marked simplicity. In common with many simple formulations it can be made to fit many situations. Rignano undertakes to carry it through all types of reasoning known to him. And in all cases he finds it adequate to his own satisfaction in describing the reasoning processes which he finds. But another person, examining the theory without the inventor's prejudice in favor of it, may often feel that the simplicity of the formulation has been misleading and that the whole analysis sometimes falls short of a satisfying account of the processes contemplated. All the while one can not avoid a certain uneasiness in view of our feeling of greater certitude about a mathematical truth, for instance, than about an empirical fact in physics. Moreover, on his theory there is difficulty even in so simple a matter as the theorem about the angles of a triangle in view of the fact that experiment must deal with particular triangles and measurements can be at best only approximate whereas the theorem itself is apparently to be accepted as a precise and universal truth.

§7. *Difficulties with Rignano's Theory.* But the difficulties with Rignano's theory can be given a much more definite form. In the field of mathematics there is a place for every type of reasoning which has successfully passed a critical scrutiny and in long experience has proved to be safe in the sense of never having led to inconsistent results when accurately used from consistent propositions or propositional functions. This is the only experimental ground possible for our confidence in a method of reasoning. But sometimes we seem to see directly that a given method is valid, as, for instance, in the case of the method of indefinite descent. One purpose of mathematics is to gather in among its methods all those which lead uniformly to valid results. In fact, Keyser has given a definition of mathematics by means of its purpose, saying that, "As an enterprise, mathematics is characterized by its aim, and its aim is to think rigorously whatever is rigorously thinkable or whatever may become rigorously thinkable in course of the upward striving and refining evolution of ideas." Hence we shall expect to find in mathematics all the varieties of exact reasoning —the reasoning which has stood the test of her exact demands. It is from this fact that mathematics is well suited to afford a crucial test of the adequacy of any theory of reasoning. That Rignano has sensed this fact is evident from the very considerable space which he gives to his analysis of mathematical reasoning.

Now if reasoning is merely imagined experimentation it must be possible to carry out in actual experiment the processes employed in reasoning and with the same degree of precision as that with which they are employed in the reasoning itself. If it is admitted that the results of some reasoning are exact to the last degree of refinement and if it is admitted—as every one must admit—that experimental measurement is always approximative in character, it would appear at once that there is a lack of consonance between reasoning and experiment—at least in some cases. Furthermore, if it is agreed that experiment, up to any given time, can never have dealt with more than some finite number of things and if it is admitted that valid theorems in mathematics sometimes refer to infinitudes of things, then it would seem that the actual divergence between the two things is too great to be covered by any kind of stretching of the notion of imagined experiment.

Let us take the case of the theorem which asserts that the number of primes is infinite, a prime being an integer with no integral divisor except itself and unity. No serious objection can be made to the truth of this proposition or to its proof. And yet the number of primes which are isolated and known as such is finite. There is no actual experimental ground on which to base the confidence which we actually have in the truth of the proposition. There is no doubt that the number of primes is infinite and there is no doubt that our empirical evidence falls far short of producing a sure conviction.

A frequently given proof of this theorem about prime numbers is in the form of a reductio ad absurdum. It appears to me that no argument of this sort can be reduced to a series of imagined experiments. For the argument consists in supposing something which turns out to be impossible on account of the contradictions which are implicit in it. This impossibility appears to me to remove it from the range of experiment. It is true that Rignano undertakes to get around the difficulty of the proof by reductio ad absurdum; but he does so in a halting way which is far from carrying conviction.

If we pass to the still more interesting proof by mathematical induction we have another clear case of the impossibility of proceeding by mere experiment. Suppose that we wish to prove by mathematical induction the fact that the sum of the first n odd integers is the square of n. We first observe the truth of the theorem for the values 1, 2, 3, say, of n. Then we let k be a value of n for which the theorem is true, not specifying what number is denoted by k. Since k is not specifically defined it is clear that we cannot experiment with it in any actual way. To say that we experiment with it mentally is mere nonsense. We can not experiment with it mentally since it is not specifically defined. What we know about k is that it is a value of n for which our theorem is valid. We know a property or a quality of k. Now we cannot experiment with a property or a quality; but we can reason about it. This we do. In fact, we prove that if the theorem is valid for the value k of n then it is also valid for the value k+1 of n. Then we employ the recursion to pass from the value 3 of n to the value 4; then from 4 to 5; and so on indefinitely. Thus we establish the general truth of our theorem.

It will be observed that an essential part of the argument is made by means of a property or a quality of an unspecified number denoted by k. This element in the reasoning removes it quite beyond the range of experiment, whether imagined or actual.

§8. *Reasoning in the Case of a Doctrinal Function.* The inadequacy of Rignano's theory will best be seen by bringing it into relation with the notion of doctrinal function. Reasoning in the development of a doctrinal function is the most typical and characteristic of all mathematical reasoning. It is of such sort as easily to elude one who looks upon mathematics from the outside. It is not surprising, therefore, that Rignano has failed altogether to take account of this type of reasoning.

We shall not repeat what we have said in earlier chapters about the nature of doctrinal functions, but shall proceed immediately to apply the previous conclusions to the matter now in hand. The postulates of a doctrinal function are themselves propositional functions, as we have seen; that is to say, they are statements having the form of propositions, but they are not propositions (strictly so-called) owing to the fact that they contain real variables. These real variables refer to elements and relations which are not explicitly defined. There are no tangible things or even images of thought that can represent them to the mind. They may be capable of an infinitude of particular interpretations and the argument must be valid at once for all of these. It is clear that no sort of imagined experiment can possibly carry the reasoning in such a case as this. There are no specific objects which can be seized upon by the imagination. There is no conceivable way by which a process of experimentation can be carried out so as to yield the general conclusions involved in the theorems.

It is true that one may experiment with particular cases; and in this way one may be led to suspect numerous theorems. But the proofs of these theorems must be independent of every particular instance. They must apply once for all to all particular interpretations. But, more than this, they must also be valid for the general abstract situation. The investigator is working with the forms of propositions and he is determining what other forms of propositions are implied by the given forms. The validity of the implications and conclusions depends merely on these forms. After the theorems are proved, one can give to the undefined elements and relations any interpretation for which the postulates are true, and then the theorems are true for the same interpretation.

This point is so important that we must insist upon it with care. One may derive at least a part of the theorems in a doctrinal function before he knows any interpretation for the function. This we saw in chapter III in connection with an account of an experiment made with a doctrinal function whose postulational basis was laid by one group of persons and whose theorems were discovered by another group. It is true that the second group found an interpretation for the doctrinal function; but it was in the nature of things that they could do this only after having discovered a part of the theorems belonging to the doctrinal function.

What happens more frequently in practice is something different from this. To begin with one knows at least one interpretation of the doctrinal function which he is developing. On a superficial examination one might be inclined to say that the investigator used this interpretation to furnish the ground of his imagined experiments—if he seeks an explanation along the lines of Rignano's theory. That this is not true we can see in several ways. From the experiment referred to we see that one can reason to theorems without knowing an interpretation. Such cases occur repeatedly in investigation, as every one must be aware who gives attention to his processes of thought in dealing with doctrinal functions. Moreover, when an interpretation is known and is held in mind, the investigator is well aware that he is reasoning by means of the forms of his propositional functions and not by means of images or imagined experiments. His argument is sustained by certain qualities or properties of the undefined elements and relations.

Moreover, there is an important indirect evidence

that one is not reasoning by means of the known interpretation. If that were the carrier of the argument one could not avoid conclusions which pertain to the interpretation in mind. In the case of finite groups, for instance, if he had a particular group in mind, its properties would appear in some of the results; but this is not the case, the results obtained apply to all finite groups. It has often happened that a new interpretation of a doctrinal function has been discovered after a large development of the function. And it is never found necessary to modify the theorems so as to allow for this new interpretation. They are true for it when it appears. This could hardly be so if the reasoning depended in any way on the interpretations known at the time when the theorems are discovered. No adequate explanation of this permanence of the theorems of a doctrinal function as new interpretations are found seems possible on any ground other than one which is based on a conception of the reasoning as being carried forward by means of the forms of the propositional functions that are employed.

No theory of reasoning is sufficient which does not account for the processes involved in developing a doctrinal function. Rignano's theory, suggestive as it is, and important as it is in many respects, is insufficient to cover the facts in this case. We do not have a theory which yields a reasoned explanation of this type of reasoning. Yet it is typical of mathematical reasoning. In fact it is the most characteristic type found in mathematics. We have set forth briefly the facts in the case. An explanation in terms of psychology remains to be found, and we must confess that theory is yet lacking in completeness. It may be impossible to give a full analysis of such a thing as reasoning. It may be, if our analysis approached completeness, that this very fact of itself would release reasoning to a new conquest. And then we should always be left without a reasoned account of our highest reasoning. We could understand it only by a sort of direct intuition or direct sympathy with the facts. At any rate, the existence of doctrinal functions and our experience with them show us that we have not yet succeeded in forming an adequate and comprehensive reasoned theory of reasoning.

CHAPTER IX

THE LARGER HUMAN WORTH OF MATHEMATICS[8]

Mathematical thought has exercised over my spirit a fascination which is far-reaching in its effect upon my activity and happiness. It is not an easy matter to present the characteristics of this thought to one who has not been initiated into the remarkable secrets of the science; indeed the difficulty is so great that the task has seldom been attempted and not always with happy consequences. And yet I have felt that I could not fail of moderate success in this matter if I could reflect with any skill the enthusiasms of my own delight, since I believe that the fire of natural and spontaneous interest in one mind has the quality of producing a corresponding exaltation in another.

There are elements of mathematical thought which illuminate my spirit with a brilliant radiance whose after-images are pleasant to contemplate. Perhaps I can not bring to you now one of these moments of illumination of joy, for one can not produce them to order or easily recapture them but I can hope to present certain after-images which show some qualities of the original.

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[8] This chapter has been allowed to stand essentially in the form in which it was delivered as an address before an assembly of the College of Liberal Arts and Sciences at the University of Illinois.

The impulse for the advancement of knowledge which is both most fundamental and most far-reaching in its practical and ideal effects is that which grows out of the pursuit of truth for truth's sake. What do we mean by this? What do we mean by mathematics for its own sake? It is clear that we do not intend to set up mathematics as a monster which must be worshipped, whom it is our duty to delight with the incense of human sacrifice. We mean rather to direct attention to the human values which are inherent in it apart from its use as a tool in any of the varied ways in which it may be so employed. Our purpose is to focus attention upon its primary values, those which it has in and of itself, those which are intimate to its own character and do not depend upon its uses outside of its own domain.

We are fundamentally so constituted that we delight in knowing for the sake of knowing. It is our most abstract and our most general motive in science. It actuates most powerfully our choicest spirits, moving them sometimes with a fervor akin to that of religion. A marvelous curiosity to know, insatiable and always demanding further satisfactions, creates a longing which can be relieved only by knowledge. It projects itself into the unknown and leads the researcher in ways yet untrodden to a goal which cannot be foreseen. At the outer boundary line of knowledge, faint glimmerings may be detected in the darkness of ignorance beyond. What beckons us forth we do not know. Whether it can bring us any good we have no means of foretelling. It may lead us to a tragical something which will make it necessary for us, in much pain, to cast away

some of our most cherished prejudices. But, whatever lies beyond in that which is concealed from our present vision,

> We work with this assurance clear,
> To cover up a truth for fear
> Can never be the wisest way;
> By every power of thoughtful mind
> We strive a proper means to find
> To bring it to the light of day.

§1. *Systematic and unsystematic Thought.* In its further reaches mathematics is perhaps the most abstruse of our mental disciplines; but in its first stages it is the simplest of those sciences which have attained permanence of result. Mathematics is the field of thought in which permanent progress is easiest. It has obtained this facility through abstraction. The problems of nature are complex beyond our ability to cope with or perceive. In the first attempt to make progress in the way of definite conquest we must abstract from the complexity of the situation and attain to a new one relatively much simpler. In fact we may find it necessary to create a new situation having certain analogies with the actual one of nature but being so much simpler that we are able to grasp far more successfully the interrelations of its parts. It is precisely this procedure which has guided the development of mathematics.

It is not that the mathematician refuses to be interested in the immeasurable complexity of nature. It is rather that he seeks permanence of conquest, even though it be at the expense (in the first instance, at least) of a narrowed range of use. The way in which mathematics has interacted usefully with other elements in the progress of thought justifies her method of abstraction as profitable; it certainly conforms to the requirements of esthetic delight for the mathematician himself.

But the abstractions of mathematics leave us in a rarefied atmosphere too far removed from concrete experience to be a satisfactory resting-place for the mind of an inquisitive organism like man. He seeks to get closer to concrete phenomena. But, unless he is content to deal in vague and uncertain generalities, he finds the complexity of nature far too great for him even though he has forged a mathematical tool to assist in his labors. He must still confine attention to certain groups of phenomena abstracted from their surroundings. He must try, so to speak, to lift them from the matrix of their environment. Thus we arrive at the exact sciences of natural phenomena, as, for instance, the science of mechanics, through the use of abstraction as a necessary preliminary to exact and permanent intellectual conquest.

But it is clear that we do not understand even these restricted ranges of phenomena which we have separated in thought but not in reality from their environment until we have considered all the elements in that environment and have synthesized the disjoined knowledge of its parts into a comprehensive understanding of the whole. When we come to these questions of greater complexity we feel less certain of our results and far less confident of the permanence of our conclusions. The history of philosophy with its changing systems and the flux of its emphasis is a striking commentary on the difficulty of the general problem.

And even here in the comprehensive problems of philosophy itself large abstraction has already been made from the complexity of phenomena and life and existence.

In definite contrast with the systematic thought of mathematics, the natural sciences, and certain parts at least of philosophical truth and speculation, stands the unsystematic thought of art and literature. Here one deals with the actual complexity of life and even with the character of individuals and their emotions. "It is the privilege of art to represent at a glance the whole of its object, and thus to produce at once a total effect on the mind of the beholder." Not infrequently men of science have seemed to overlook the importance of this body of unsystematic thought in art and especially in literature. But it appears that the development of unsystematic thought is necessary to sanity; not that its unsystematic character as such contributes to this end, but that through no efforts being made at systematic statement one can allow the whole flux of life to be reflected at once, at least so far as to have no purposed exclusions. If one is to have the systematic exactness of pure science it can be only after many relevant considerations are shorn off and attention is fixed on a part only (usually a small part) of the whole. This is necessary to definite conquest and the method is to be freely used. If one stops with this, however, a one-sided unbalanced view results which contributes forcibly to a lack of sanity in outlook and general judgment. With the continued development of systematic thought, let us encourage and support the free development of unsystematic thought in poetry and other forms of literature and art. The latter have abiding qualities of intrinsic human worth which science can never replace.

The domains of systematic and unsystematic thought have usually little effect the one upon the other; and yet between these two great arteries of our culture there must be somewhere a vital connection. The historians of general thought have not yet properly taken into account the vast body of unsystematic thought in the literatures of the world where for millenniums it has awaited their research. Nor on the other hand has the poetry of exact truth been written nor have its cultural elements in any representative case found their way into the general thought of mankind.

Literature not only takes the complex whole of life at a glance but it also internationalizes its local subjects and gives them a value which endures independently of time and place. Mathematics and poetry lie, if not on, at least not far from the extremes, the one of systematic and the other of unsystematic thought, and thus are about as far removed as possible one from the other. And yet they have a very striking common property, namely, the property of permanence. No other large domains of thought than mathematics and literature have acquired large bodies of truth retaining their values essentially unimpaired for two thousand years, not in a stagnant state, but in a state of vitality and effectiveness. It is a matter of great inspiration to see the Greek geometry and the Greek tragedy surviving through the ages and retaining the active power to excite our admiration and increase our happiness today.

§2. *The Language of exact Truth.* Communication to others requires the previous construction of a language having the requisite flexibility. If we look into the remote origins of our culture we shall find reason for believing that it was language which initiated the marvelous release of the powers of man inherent and undeveloped in our primitive ancestors and coming to their fruition only after many ages of progress. It was and is a fundamental element in accumulating and retaining the heritage of the past so that each new generation, in periods of development, is able to begin a little further on than the preceding one.

There is something subtle in the way in which language makes it possible to pass the experience and thought of one generation along to the next. The phenomena of nature present themselves to us ordered in space and time, but without apparent logical connections to bind them together. As long as we meet them merely in the multiplicity of their separate existences we can not get far towards an understanding of them or of a mastery over them. It is necessary that they shall be ordered into groups or sets, each held together by some tie which serves in our mind either as a unifying or as a connecting element. The combination of distinct elements into a whole and the formation of these groups depends on a process which the mind constructs for itself slowly and only after much labor. Any means of giving a considerable measure of permanence to the constructions of one individual mind or of one age, will be of great value in maintaining mastery and effecting its further development.

Let us conceive, if possible, the condition of prehistoric man at a time when language was in the process of construction for the first time. When a tribe of men reach agreement concerning the common elemen of a set of objects, as for instance the trees of the fo-est, and signalize a realization of their common features by giving to them some such name as tree, they crystallize into definite form a class of experiences felt by each of them in a more or less vague way. The idea denoted by the word becomes more distinct by constant recurrence and both word and idea take their places as part of the mental possessions of man.

This primitive process has been repeated in all ages of our history; it recurs often in the present day, notably in connection with the development of scientific thought. In youth we listen to the words of those of the previous generation, trace in their features some mark of the anxieties through which they have lived, and share remotely their enthusiasms and aspirations, their passions and their joys. But we receive through them in the language which they teach us a more living inheritance and a more eloquent testimonial to their ways of thought. "Unknowingly they have themselves altered the tongue, the words and sentences which they received, depositing in these altered words and modes of speech the spirit, the ideas, the thought of their lifetime. These words and modes of speech they handed down to us in our infancy, as the mould wherein to shape our minds, . . . as the instrument with which to convey our ideas. In their language, in the phrases and catchwords peculiar to them, we learnt to distinguish what was important and interesting from what was trivial or indifferent, the subjects which should occupy our thought, the aims we should follow, the principles and methods which we should make use of."

A word or a way of thought into which so much experience of the race has been instilled can easily be taught to the children of a new generation and be made to serve for them as a nucleus about which they can gather experiences of their own similar to those first embodied in the language. Thus through the various words which they use and the various turns of phrase which they employ they have a subtle means of assistance in organizing their early experience so that they are able to make much more rapid acquisition of knowledge than their ancestors who first had the confusion of unorganized impressions out of which to construct the initial organization of truth.

To the individual who is brought up in a civilized and intellectual age words and their organization into sentences certainly come earlier than clear and conscious thought. Through the use of our parents' tongue we are introduced to the complex processes of highly abstract reasoning in a manner which is truly marvelous. The way of thinking of our ancestors, preserved in some measure in the constructions of their language and in the peculiar ways of expressing thought developed through ages of progress, becomes to us our most precious heritage from the past. A highly significant part of the development of mankind is summarized into the forms and words of language in such a way as to be capable of transmission and to be of unmeasured value in passing on to the children the acquisitions of their ancestors.

What the language of daily life does for the thought of usual intercourse the language of mathematics does for the thought of exact truth. Everything which I have said about language in general I can now transfer to the language of mathematics in respect to its use in connection with exact thought. It furnishes the essential means for the expression of the facts. It supplies the support without which the mind would be unable to carry through the processes necessary to attain the more profound or far-reaching results. There is a certain storing, as it were, of intellectual force in the mathematical symbols from which it can be released suddenly with almost explosive power. These become mighty engines through the aid of which we can rear intellectual structures quite inaccessible to our unsupported power.

The invention of number was the first step in the creation of the language of mathematics; and the choice of adequate and convenient symbols for the representation of integers is one of the chief triumphs of the intellect. A long and arduous mental struggle, in which some of the finest minds of antiquity had part, preceded the conception of zero and the introduction of a symbol to represent it in the way now familiar even to our children. The result of a long and important development of thought is embodied compactly in this remarkable sign. The introduction of a symbol like 4/5 marks a new stage in the development of mathematics. The general fact is repeated in many situations; but I cannot go into a further analysis of this matter. It is sufficient to our purpose if we realize that the language of mathematics is an essential support to the mind in all its processes of exact thought and that the results emerging in this way can be expressed only in mathematical terms.

Being a lover both of mathematics and of poetry I enjoy finding certain general similarities between them. I have already alluded to one very striking common property of them, namely the property of permanence. I wish now to direct your attention to the historical fact that poetry was the primary and most important means by which the language of ordinary intercourse was brought to a stage of relative perfection just as mathematics was the essential means in creating the language of exact truth. Ordinary language having been brought to perfection by the labors of the poets was then appropriated by the writers of prose; exact language having been developed by the mathematicians has been employed freely by the cultivators of every exact science.

§3. *Mathematics and Philosophy.* Philosophy and mathematics started life together. After a brief period of companionship they parted company and each went its own way. Mathematics was the first science to emancipate itself from the tutelage of philosophy; it gained its freedom at the dawn of Greek civilization. Mechanics next succeeded towards the close of the Grecian period, physics obtained its independent position at the opening of the modern era, biology about the beginning of the nineteenth century, and psychology in its latter half. Sociology as an independent science has hardly yet passed its period of infancy.

When men began consciously to cast about them to understand their universe, they found it possible in a relatively short time to procure and contemplate a large body of unsystematic thought, a wealth of philosophic explanation, and a rather large body of speculative proto-science of nature. But in respect of mathematical knowledge they had to begin much nearer the bottom. In the less exact disciplines there was an ebb and flow of movement with a general progress forward, accompanied often by a discarding of what at one time was considered well established. But in mathematics a conquest once made is almost never lost and there is a consequent unbroken enlargement of doctrine. Since it pushes its conquests out in many directions, is frequently annexing new domains, never yields up what it has once attained, and remains youthful in its spirit of conquest, mathematics is destined to become, if indeed it is not already, the most extensive scientific doctrine in the whole range of knowledge.

Early in the history of thought philosophy soared the heights on wings of speculative grandeur and soon reached an eminence which it has never surpassed. Mathematics took time to dig deep till it was in possession of secure foundations on which to build. Here it reared a magnificent structure of enduring beauty. In our generation this mathematics has reached forth a hand of conquest and has annexed certain restricted domains of philosophy. "The first real advance in logic since the time of the Greeks was made independently by Peano and Frege—both mathematicians" working with the tools and from the point of view of mathematics. In former days the nature of infinity and continuity belonged to philosophy, but now it belongs to mathematics. An important part of the theory of classes has been annexed by this greedy conquerer.

But more than all this, it has injected its spirit into a large province of modern philosophy. Among the philosophies of the present day Bertrand Russell distinguishes three principal types, combined in varying proportions in single philosophers but in essence and tendency distinct, namely, the classical tradition, evolutionism, and the method of logical atomism. The last has crept into philosophy through the critical scrutiny of mathematics. According to Russell it represents "the same kind of advance as was introduced into physics by Galileo, the substitution of piecemeal, detailed, and verifiable results for large untested generalities recommended only by a certain appeal to the imagination."

A doctrine which lay quiescent in the domain of philosophy for many generations has recently been brought by mathematical methods into the activity of vigorous life. The modern theory of relativity is a precise physico-mathematical realization of the philosopher's speculation of relativity. The existence of the philosophical doctrine has been of profound value in the creation of the mathematical doctrine; but the latter is now so far in advance of the former that the philosopher is rare who is able to follow the train of thought by which the more exact theory is brought to fullness. This mathematical conquest of a domain of philosophy has in our generation yielded a penetrating insight into certain fundamental matters both of physics and of philosophy. A theory of gravitation, satisfying for the most part in its broad aspects, has come into being for the first time. Under the impulse of this theory our notions of force and mass have suffered considerable change and our conceptions of time and space have undergone a veritable revolution.

Without going into more details in these matters, I may insist upon the fact that one of the profound intrinsic human worths of mathematics lies in its conquest over intellectual matters of perennial interest on which agreement cannot be reached until they are penetrated by the spirit and methods of mathematics and the invariant elements of truth are extracted and justified by a convincing array of precise evidence.

§4. *Mathematics and the Foundations of Science.* Mathematics is autonomous. What is intimate to it, its nature and structure and laws of being, must be sought in itself. Logically the mathematical sciences can be developed in complete independence of all other sciences; and when pursued in this way to their goal they completely realize their object. Owing to its self-sufficiency, its abstract character, and its exclusion of complicating factors from the ideal considerations with which it is concerned, mathematics is essentially easier than the other branches of systematic truth. The appearance of greater difficulty, which has deceived most people, grows out of the fact that it is relatively further advanced than any other subject. It requires the learner longer in mathematics than in other sciences to attain an elevation from which he may enjoy the prospect of unexplored territory. Its wildernesses are further from the confines of civilization; and the ignorant picture them as filled with horrid monsters of indescribable physiognomy. But the hardy intellectual traveler who explores in this land of the far unknown finds nature gracious, there as well as here, in dispensing her beauties and joys and comforts.

As a discipline which is unique through its being more completely developed than any other it may be

utilized as an object lesson of importance in the development of thought. The mind has not been able to chart unknown regions and to explore them systematically. Truth, when attained, often has an appearance quite unexpected. Its central characteristics can not be anticipated before it appears in thought. Consequently, the extended development of any discipline affords a means of analyzing the methods and foundations of successful thinking and of extracting by such analysis principles of guidance for all domains of exact thought. In certain important respects mathematics affords just such a support to the mind in finding its way to truth: it has continuously rendered a service of this character since the days of the early Greek philosophers. This contribution has varied in detail from age to age, ranging from marvelous uses in interpreting physical phenomena to marked support in speculative philosophy and the theory of knowledge.

During the last half century or so mathematics has come definitely to a stage of self-consciousness with respect to its processes and presuppositions; and these have been analyzed and subjected to critical logical scrutiny. The foundations on which the subject is built are understood with a completeness foreign to any other domain of thought. From this fact it may well serve as a matter of instruction to point the way to a suitable and needed analysis of the presuppositions on which any given principle is founded.

The importance of such an analysis seems not always to be apprehended. The sciences of nature are shot through with presuppositions not recognized.

Even in the more precise reasoning of mathematics there was much to be elucidated by a critical scrutiny; and certain presuppositions had to be brought into the focus of attention before it could be properly said that we understood the foundations of the science. Elsewhere such analysis has been made only very imperfectly; the success achieved by mathematics in this work has not yet borne its proper fruit. It has been made clear that no science has been brought to a truly objective stage in its development until the presuppositions lying back of it are perceived as such and the grounds for making them are clearly realized. In the sciences of nature this process is more difficult than in mathematics; this, indeed, accounts for the fact of the earlier success in mathematics than elsewhere. But when the result is once achieved in one science no other should rest satisfied until the same end is reached in an appropriate way.

§5. *Mathematics and the Method of Thought.* Perhaps it will be agreed that we can nowhere study the processes of thought, by which the intellect reaches appropriate decisions, more effectively than in that domain where it has been most successful in attaining enduring results of significant value. If this principle is agreed upon, it is a corollary that mathematics is a field of thought which will yield us some of our most definite information as to the essential elements in the methods of clear and accurate thinking. Unfortunately, mathematicians themselves have generally been but little interested in the broad principles of method which their achievements are capable of bringing to light; they have usually been disposed to stand apart from the

broader questions of a theory of knowledge, satisfied apparently with the self-sufficiency of their own discipline. Outside of their fold there has never been a group of thinkers with the requisite information and training to elicit from the body of mathematics the instruction which it is thus capable of affording. This field of promising possibility lies uncultivated while we lack those advantages which its fruitage well may yield.

It is important to ascertain the character of those regions of thought in which new methods have most frequently arisen into clear consciousness. Owing to precision of ideas and processes in mathematics we can answer that question definitely and with considerable confidence for that discipline. New methods have usually come to light in connection with well-defined and well restricted problems. Experience has forced upon us a realization of the profound importance of deep penetration into even the simplest matters. When a new means of illuminating them has been discovered its radiance spreads to adjacent fields and often overleaps great barriers to shed new light in most unexpected places. The connections between different elements of thought can not be anticipated successfully; it requires the event to exhibit them. The presuppositions which underlie truth become apparent gradually as we derive the remotest consequences of what is already known. For the researcher everywhere the character of the success in mathematics emphasizes the importance of detailed and penetrating and carefully analyzed investigations of basic matters.

The continuous advance in the understanding of the presuppositions of our science, the axioms or postulates on which it rests, and the resulting modifications in our views of its significance impress us constantly with the supreme necessity of the logical coherence of knowledge. No principle is thoroughly understood until all of its consequences are developed and their ramifications are ascertained. This process can be carried out only through the most searching logical scrutiny; it is desirable that the intuition shall be present in discovery, but the logical faculties should dominate exposition completely. "To supersede the employment of common reason, or to subject it to the rigor of technical forms, would be the last desire of one who knows the value of that intellectual toil and warfare which imparts to the mind an athletic vigor, and teaches it to contend with the difficulties and to rely upon itself in emergencies." But when its results are once attained and they are to be put to the test of a systematic organization for determining the coherence and consistency of the parts, no glow should be permitted except that which comes from the cold light of logic.

A more deep-lying problem of the method of exact thought is brought out by the question as to the fundamental character of that mental process by which scientific truth is discovered. Natural science always proceeds in one of three ways: mathematically, experimentally, or by hypothesis. Have all these methods fundamental matters in common one with another and with the processes of mathematics itself? And, if so, are they of such sort that it is useful to the progress of science or to our delight in it to have them brought to

attention? Owing again to the relatively more advanced state of mathematics as compared with the natural sciences we can consider for it, in a more objective way than for them, this question as to the basic characteristics of the process of discovery.

Not a few mathematicians are agreed that these characteristics are summed up in considerable measure in the word invention. Some of the things in mathematics one may think of as being discovered; but others, and the more fundamental things, seem to have been created by the mind. The positive integers, for instance, were not found in nature but were created by the human spirit. After their creation many of their properties have been discovered. This relation between invention and discovery pervades most of the mathematical literature. Mathematical space has been created, not found in nature, as is shown by the fact that the mathematician has several kinds of three dimensional space as well as numerous spaces of higher dimensions. It is true that his creative power was released through observation of the environment; but it can not be maintained that the environment dictated the geometry since in that case only one geometry could have resulted. A full analysis of the matter would carry us much too far afield, but we may assert that the process of discovery in mathematics is primarily that of invention.

This leads the mathematician to suspect that the method of exact thought everywhere is largely dependent upon invention, that the hypotheses of science are not extracted from nature but are invented by the mind through a release of its powers brought about by natural phenomena. Since one's procedure in forming hypotheses is doubtless much affected by his conception of the nature of the process it is important that the laborers in each science shall ascertain the corresponding fundamental characteristics of their process of discovery.

If we should suppose that the advance of knowledge among the most cultivated people is in the direction of making life not worth while this would operate to destroy the part of society so affected with pessimism and the whole earth would ultimately be left to the less advanced. Thus a philosophy which makes life not worth while will have a natural tendency to destroy itself, so that it can not become permanent. That philosophy of the method of thought which results from a contemplation of mathematical progress leads to a doctrine which dignifies the process of thinking, exalting it to a place of veritable creative grandeur. It proceeds in the direction of making life worth living. It is optimistic in outlook and thus has one of the first qualities which are essential to permanence.

§6. *The Invariants of Human Nature.* Another value of mathematics is its creation or clarification for its own use of various concepts which are afterward seen to serve as a unifying element about which other large domains of truth may be systematically organized and the relations of fact thus be brought to clearer understanding. Everywhere we are confronted by change; nature seems to be in an eternal flux. The complexity of particular phenomena is bewildering and we should be lost in their maze if we could not find some means of ascertaining the elements of permanence

in the midst of the flux. In mathematics we have the same situation freed of distracting elements and idealized in a way which makes it possible to give a rather complete analysis of the whole matter. The flux and change of nature is replaced, in the ideal situations of mathematics, by what we call a group of transformations. The elements in consideration are subject to transformation according to the laws prescribed by the group which governs the phenomena; and our problem is to determine the things which are unchanged in the midst of the general flux allowed by the controlling group; in other words, we are seeking what we call the invariants, or the invariant combinations, of the elements subject to the flux permitted by the group. This conception, vaguely present in much of scientific speculation, has been recreated by mathematics into precise form, has been clarified, and has been utilized so fully that we now find it to be true that a large portion of the whole of mathematics has to do consciously or unconsciously with the theory of invariants. The essential elements of the logical characteristics of a situation of this sort are brought out clearly by the mathematical theory. The resulting body of truth furnishes us with a model by which we may be guided in the contemplation of the elements of permanence in any changing situation.

Whatever the subject of inquiry in any domain of exact thought there are certain entities whose mutual relations we desire to ascertain. The combinations which have an unalterable value under the changes to which the entities are subjected are their invariants. It is the purpose of the theory of invariants to determine these combinations, elucidate their properties, and express in terms of them the laws which are involved in the given situation. The "laws of nature" are expressions of invariant relations under the changes occurring in nature or brought about by directive agency. Two problems concerning natural phenomena demand attention. If we know the group of changes we may demand the determination of the invariants; if we know the invariants we may demand the determination of certain (if not all) possible groups under which these invariants persist. To enforce the judgment that invariants are a fundamental guide in present day science we have only to cite the fact that the theory of relativity has been developed in intimate dependence upon and under the guidance of the theory of invariants.

To pursue this matter further would carry us too far in the direction of a study of the usefulness of mathematics in the development of natural science, a matter which we are purposely excluding from present consideration. It has been said by them of old time that the proper study of mankind is man. Our purpose keeps us closer to this problem than to the study of nature. It is a fair question to ask what mathematics has to teach us concerning human nature. What do we mean by human nature except those characteristics of individual people which are unchanged from one to another, and from age to age? Those elements which are invariant through the whole group of human beings as far as they may be brought under observation? And how shall we determine the characteristics of this human nature other than by an analysis of the invariant elements in human experience and thought?

It cannot be maintained that mathematics affords the best means for pursuing this study. In fact, it is probably generally supposed that mathematics makes no contribution at all to the problem of human nature, of the invariants among the qualities of individuals; we shall attempt to show that this judgment is incorrect.

The best means of studying human nature of course arises from the usual relations of life. But these in themselves are quite insufficient for a complete analysis. The continued acceptance of a large body of vital mathematical truth through some millenniums suggests the invariant character of certain elements of human thought in its logical aspects, just as the continued appeal of ancient poetry (for instance) to people of cultivated taste bespeaks an unchanging element of human nature in its finer emotional aspects. The presence of such invariant elements, wherever they may be found, is an instructive matter for the historian of culture and civilization.

Where can one find a systematic analysis of literature, that great storehouse of material for the understanding of human nature and the progress of unsystematic thought, having for one of its primary objects the ascertainment of the invariant elements of human nature in its emotional aspects? A study of the changes in taste and their cause contributes indirectly to this end; but both literature and mathematics, in different ways and with reference to different parts of our nature, can be made to yield important values towards an understanding of its own invariant elements.

Since the historian of thought and civilization is seeking to bring his analysis of the progress of culture into systematic form it is perhaps no great surprise that he has found it difficult, and so far has not found it possible to utilize successfully the truth which is half-concealed and half-revealed in the unsystematic thought of literature. But it is rather astonishing that such historians of thought have not been able to utilize the systematic work of mathematics in their expositions. I know of only a single instance where a general analysis of the progress of thought has taken an adequate account of the domain of mathematics, and that is in the work of J. T. Merz on *The History of European Thought in the Nineteenth Century*.[9] This excellent general analysis has not had for one of its purposes to bring out the invariant qualities of human thought, and hence of human nature, as they are made manifest by the abiding truths of mathematics. The contribution which mathematics has to make to the study of human nature has not yet been considered in a systematic way.

And still it is certain to those who contemplate the nature of mathematical truth that many characteristic qualities of human thought are to be determined from such an analysis. It is a significant fact for the understanding of ourselves that the demonstrations in Euclid's *Elements* gain the same adherence today as in his time and in all the intervening ages; an invariant quality in the processes of reasoning and the ground for conviction through demonstration abides through

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[9] To this magistral work and to many articles in the *Encyclopaedia Britannica*. I am under deep obligations in connection with this chapter. I have also profited by reading C. J. Keyser's *The Human Worth of Rigorous Thinking*, Columbia University Press, 1916.

the ages. There is absolute agreement in all times and all places that the number of prime integers is infinite, bespeaking a unity of the whole race in its understanding of the properties of the elements conceived in the first place with exactness. The properties of the Euclidean triangle are in harmonious agreement even though they have been discovered by numerous thinkers of many generations. A sphere did nothing for the Greeks contrary to what it does for us today. The properties of a cube are invariant, whoever derives them and in whatever age he lives. It is an eternal truth that every integer is the sum of squares of four integers, and there is unanimity as to this fact and as to its demonstration. The persistence of mathematical theorems and the continued agreement as to their proof indicates a profound unity in the characteristic thought processes of those who contemplate them, exhibiting one fundamental phase of human nature.

§7. *Artistic Delight in mathematical Truth.* Truth serves many ends. When a science has reached a certain stage of development, varying greatly with the character of its material, it begins to throw off into the body of society great practical or even esthetic values which could not be realized without it. Astronomy has enabled us to have some conception of the vastness of space and the hugeness of the mass of matter, perhaps infinite in its totality, distributed through this space. Geology has released the imagination to contemplate enormous periods of time and, through its influence on biology, has rendered marked service in making possible our conception of the long progress of life on the planet, culminating in man. Mathematics, by exhibiting a body of truth which can live through millenniums without needed corrections, and at the same time can grow in magnitude and range and interest, has given the human spirit new ground for believing in itself and for rejoicing in its power of consistent thought.

It is not enough to accumulate the elements of knowledge or even by means of them to control nature for our use; we must appropriate them by idealizing them into things of beauty and motives to conduct. Truth may be made to yield the highest delights of contemplation in the spirit of artistic performance. This is generally realized in the case of the unsystematic truth afforded by literature and the other fine arts. It is less in evidence in the greater body of systematic truth. But when the latter is brought to its highest order of perfection, as it is in the domain of mathematics, it becomes capable of yielding the purest and most intense delights in artistic excellence. They are of a sort to be enjoyed in large measure only after an adequate training; and in that respect there is a certain exclusiveness about them. But to those who are willing to pay the price of adequate knowledge mathematics yields a gratification of the artistic sense surpassed by that arising from no other source. "The musician plays and the artist paints simply for the pure love of creation." The mathematician creates abstract and ideal truth for the pure love of discovery and of contemplation of the beauty of his mental handiwork.

In pursuing esthetic satisfactions we create a beautiful theory for the sake of our delight, as in the case of the theory of numbers or of abstract groups. Working in such fields with the simpler elements of

mathematical thought we make progress of a sort not at first possible with the more complex materials. We bring the theory to a higher state of perfection; there are fewer lacunæ; the connections of the various parts are exhibited with clarity; we have a sense of having seen to the root of the matter and having understood it in its basic characteristics. The theory thus developed becomes an ideal in the light of which we get a new conception of what should be attained in other fields where the labor and the difficulty are greater. Results in one field of mathematics may thus become of great value in a totally different range of mathematical ideas or even in other disciplines altogether. Moreover, when such progress is attained we often find that the tools employed in bringing it about are sufficient for dealing with more difficult matters, so that the one completed theory furnishes us not only the ideal, but also the means, for further valuable progress.

A characteristic delight in mathematical truth is that which arises from economy of thought realized through the creation of general theories. When we develop the consequences of a set of broad hypotheses we find that our results, which are attained by a single effort, have applications at once in many directions. Thus we see the common elements of diverse matters and are able to contemplate them as parts of a single general theory pleasing for its elegance and comprehensiveness.

Fundamentally mathematics is a free science. The range of its possible topics appears to be unlimited; and the choice from these of those actually to be studied depends solely on considerations of interest and beauty. It is true that interest has often been, and is to-day as much as ever, prompted in a considerable measure by the problems actually arising in natural science, and to the latter mathematics owes a debt to be paid only by essential contributions to the interpretation of phenomena. But, after all, the fundamental motive to its activity is in itself and must remain there if its progress is to continue.

"The desire for the one just form which always inspires the literary artist visits most men sometimes" and is ever present to the mathematician in his hours of creative activity. The one just form which the mathematician seeks is more ideal and perhaps more delightfully artistic than that sought after by any other thinker; for it is primarily a form of abstract thought in which he is interested, a form which remains the same as ages come and go, as languages are developed and die away, as the canvas of the painter rots to fragments and the material of the sculptured image is resolved by decomposition into its elemental dust. It is a thing of beauty which is indeed a joy forever.

For many people the numerous practical applications of mathematics have obscured its artistic elegance. But it is not the only fine art which in another aspect is also of the greatest practical utility. This quality it shares with the noble art of architecture. The two equally satisfy the following informal definition given by Sidney Colvin: "The fine arts are those among the arts of man which spring from his impulse to do or make certain things in certain ways for the sake, first of a special kind of pleasure, independent of direct utility, which it gives him so to do or make them, and next for the sake of the kindred pleasure which he derives from witnessing or contemplating them when they are so done or made by others." Both mathematics and architecture possess all the qualities here enumerated. Each is of essential practical utility, contributing necessary elements to the material comforts of man. And, more than this, each delights the artistic sense through beauties peculiar to itself and furnishing the ideal reason for its existence.

From a certain point of view the four main divisions of thought—mathematics, natural science, philosophy, that unnamed one ruling without definite system in the domain of art and literature—are the stones and brick and mortar from which is builded the culture of the time, into which are wrought the values received from the past, and through which our development shall proceed to the acquisition of new power for further conquest. We break the environment into parts in thought and from these we fashion new objects such as never before existed in the universe—objects both concrete and ideal—and these we put together in ways well pleasing to ourselves to serve the ends we propose or erect the constructs we conceive.

But this is too mechanical to be the whole truth. The more profound values lie deeper and have their fruition only in the fullness of the character of man. If science did not touch a more profound matter than mere motion or reach to constructs which can not be adequately pictured by material symbols, it would fall far short of the glory of Living Thought. But it does forcibly react in a profound way with all our activities, particularly through the emotions excited by the play of the artistic sense. In fact, the elements of all Thought are parts of one body, living and organized, inspired by the breath of the Universe itself and pulsating with the life of truth in its deeper manifestations.

§8. *The Problem of consistent Thinking.* The leading characteristic of man is the power to think. There is nothing of higher esthetic interest than to determine whether we can think consistently. This fundamental question can be answered in the affirmative only by exhibiting the results of consistent thinking. The existence of mathematics affords the best conceivable proof of its possibility and gives the spirit of man leave to believe in itself, since here admittedly is a body of consistent thought maintaining itself for generations and even for millenniums, able to sustain all the attacks of logic and all the tests of the practical life.

There was a time when this confidence in the permanence and consistency of mathematics was absolute. The fundamental method of argumentation men conceived to belong to a class of innate or inherent ideas which had been put in the mind of man by the Creator. The initial hypotheses and basic notions of a mathematical discipline they thought of as belonging to the same category. If these innate ideas did not have all the elements of absolute certainty, there could be only one conclusion: the Creator had deliberately deceived man. Since they considered this to be absolutely impossible, they had complete confidence in the certainty of mathematical results.

Nowadays we seek a more earthly reason for confidence in our constructions in science. Our agreement that mathematics is possible as a consistent body of truth we now understand to rest on postulates for

which admittedly we have no logical demonstration. Perhaps these postulates may be framed in the following way: Reasoning is possible and does not lead to wrong results when employed according to the universally accepted rules; mathematical objects can be created or discovered by the mind; we can actually formulate consistent axioms or postulates concerning these objects. With each of these three statements there are grave logical difficulties. We can not assert that we have an immediate perception of them as true; we can not, by direct illumination, see their validity. We must examine each of them in the cold light of experience and accept it only in so far as it meets the most exacting demands. Our confidence can never be absolute. As J. B. Shaw has said in his *Philosophy of Mathematics*: "We may found our deductions on what premises we please, use whatever rules of logic we fancy, and can only know that we have played a fruitless game when the whole system collapses—and there is no certainty that any system will not some day collapse!"

Let us proceed further with the difficulties of the situation. In all preceding generations conceptions in mathematics have been used with confidence which, in the experience of a later day, were found to be not sufficiently well defined; they have been discarded or essentially modified, sometimes after generations of confident use. It is not likely that men have heretofore always made mistakes of this kind and that we have suddenly come upon an age in which mathematical conceptions are refined to the last point of analysis.

We are then forced to the conclusion, however unwelcome it may be, that the certainty of mathematics after all is not absolute, but relative. To be sure, it is the most profound certainty which the mind has been able to achieve in any of its processes; but it is not absolute. The mathematician starts from exact data; he reasons by methods which have never been known to lead to error; and his conclusions are necessary in the sense, and only in the sense, that no one now living can point to a flaw in the processes by which he has derived them.

Let us make as concrete as possible the difficulties and the immense values which are at stake. Let us suppose that the Euclidean geometry should become untenable under the weight of constant accretions and should go crashing down to helpless ruin; in that day man's hope of reaching tolerable certainty anywhere in his thinking would be destroyed and even the world of mind would become a dark confusion of irrational elements. The character of such a loss suggests the magnitude of the present value.

What certainty have we that such loss does not impend? We have no logical demonstration of its impossibility; and in the nature of things can not have such a demonstration. The same uncertainty attends all other truth, and in even more marked degree. There is no logical certainty of the consistency or the permanence of truth; at most there is a moral certainty. From mathematics we have the strongest grounds for the latter. When thousands of persons through thousands of years examine thousands of theorems proved by numerous methods and in numerous connections and there is always absolute unanimity in the compelling character of the demonstration and the consistency of results, we have a ground of moral confidence so great that we can dispense with the proof of logical certainty and comfortably lay out our lives on the hypothesis of the permanence, consistency and accuracy of mathematical truth. The existence of mathematics gives the mind the best reason yet advanced for believing in its powers and the essential accuracy of its careful processes.

§9. *Emotional Exaltation arising from the Contemplation of mathematical Truth.* A profound emotional exaltation arises from the contemplation of mathematical truth either in the static aspect of accomplished results or in the dynamic aspect of a science with an everlasting urge to further development. By the ideal values which it constructs and by the permanence of its results mathematics gives to the spirit of man the right and the courage to believe in itself and to trust its controlled flights. Here it justifies its claims to preeminence more completely and more profoundly than in any other part of its broad domain. It exhibits a body of truth which is permanently pleasing and which exacts confidence at all times and among all thinkers who examine it.

By building first on its narrow and exactly conceived foundations and by adding bit by bit to its possessions of permanent truth, mathematics has made possible a release of the imagination of man such as can be completely realized to-day by only a relatively few individuals, a release however which will allow an expanse of the general human mind to-morrow or the next day. Vast new domains of contemplation are opened up by the non-Euclidean geometries, theories of hyperspace and space of an infinite number of dimensions, functions of an infinite number of variables and functions of lines. Such conquests give a new sense of power and mastery and increase the dignity of man. In the presence of so many beautiful creations of his thought "the mathematician lives long and lives young; the wings of his soul do not early drop off;" he rejoices in the grandeur of the heights to which his controlled imagination attains; he delights in the construction of truth, beautiful in its inner relations, consistent in its logical connections, and enduring by its nature and character; bit by bit, he adds to the general store of intellectual wealth in his domain; and sometimes he enlarges it by great new conquests.

If one is to realize the intenser delights afforded by the contemplation of mathematics he must of course be a deep student of its secrets. It is only when he is able to devote a large share of his energy to research and is successful in the creation or discovery of important new truth that he may rejoice in the fullest glow of delight through a realization of himself in such ideal conquests. However important the work of preserving past discoveries and handing down to the future the accumulated tradition and however far-reaching such a stream of influence flowing in hidden ways in the minds of cultivated people, it can not be placed in the same category with that creative work which guides instructor and student alike and teaches generations what to think. It is the great glory of mathematics in our time that its achievements are being immensely enriched and extended by the researches of the present;

so that this, the oldest of the sciences, has the vigor and the spring and the growth of the youngest of them. He who discovers a fact or makes known a new law or adds a novel beauty to truth in any way makes every one of us his debtor. How beautiful upon the highway are the feet of him who comes bringing in his hands the gift of a new truth to mankind.

Alone before the wild and restless force
Of nature we have seen man's active soul
Stand forth in awe without a sure resource
Of power to overcome or to control
The salient things submerged beneath the whole.
And we have seen in vision some new power
Spring up from hidden depths of mind and roll
With bounding joy to conquest, hour by hour
Increasing till the strength of man reached fullest flower.

What stage of progress have we now attained
In this process of far-unfolding thought?
What ground to think that it shall be maintained?
Have we the fullness of our conquest brought
And reached the depths where nature works and caught
From her the deepest blessing she can yield?
Or fathomed her profounder secrets fraught
With good, no major truth remaining sealed
From sight, with only minor things to be revealed?

If so, no glow of zeal could move our thought;
Our life would lose its meaning and its zest;
No vision of the future could be caught
By mind's prophetic penetration, blest
With prospect large; in pessimistic rest
And deep stagnation then must mind abide
Without a great compelling interest
To bring its power to action and to guide
Its strength to ways of joy or largest use provide.

But crescent science such a view as this
Dispels; for largest things with keen insight
We feel; the growth of knowledge must dismiss
From thought such pessimism; its darkening blight
Of shadow is illumed by sure foresight.
We joy to see new worth to be attained
And know the present conquest is but slight
Compared with wider truth that shall be gained
For thought's dominions, now by science unexplained.

We need the willing mind to consecrate
Its strength to finding truth, the zeal to bring
From nature's storehouse values good and great
And lay them at the feet of man. To wring
From restive nature some unwilling thing
Were joy supreme. The means for our release
To greater power we seek. The bounding spring
To growth shall move in us and never cease
To bring to us new joy and truth's renowned increase.

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